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## ORIGINAL LECTURES.

### ON PRIAPISM ACCOMPANYING INJURIES OF THE SPINE.

*An Abstract of a Clinical Lecture Delivered at the Pennsylvania Hospital, February 11, 1882.*

BY WILLIAM HUNT, M.D.,  
SURGEON TO THE HOSPITAL.

GENTLEMEN: In the *Medical News and Abstract* for November, 1881, I discussed in a lecture on Injuries of the Spine, the symptom of priapism which sometimes exists after those injuries, and which, when it does occur, is a sure index of what has happened, and mostly is of very serious import. I also called attention to the fact that it may exist after injuries anywhere along the column, and that I could confirm this from my own clinical experience. I then showed that the explanation is not to be sought for on the theory of centres somewhere in the cerebro-spinal axis, but that the phenomenon is due to disturbance of the vaso-motor nerves, through the direct involvement of the sympathetic and its ganglia in the injury. I also called attention to the fact that in normal erection the intensity of sexual passion, or local irritation overcomes the inhibitory function of the sympathetic system, and that this function is also overcome when the sympathetic is lacerated, irritated, or divided at the same time that the cord is injured, in the latter case erection occurring without sensation, and being more or less persistent according to the severity of the injury.

"Thus," I say, "it is not necessary to trouble one's self about a special nerve-centre ruling over the function we are considering. Thus also this symptom in accidents to the column and cord is explained; also why some have it and others do not, why it is evanescent in some, and in others almost constant, why it may appear in injuries occurring anywhere along the spine, why there is no sensation when it occurs as an accompaniment of laceration or pressure on the cord, for communication with the brain is cut off, and why, also, it is rare to find it in disease of the membranes or cord.

"When the wounding missile or crushing force breaks the spine and involves the cord, and there is accompanying priapism, then the neighboring ganglia or nerves of the sympathetic are bruised or wounded at the same time, from being caught in the line of the crush or wounding missile. When this symptom does not occur, the sympathetic has escaped. I have no doubt that when it occurs in hanging, the superior cervical ganglia of the nerve are caught in the squeeze. In most cases of disease the lesion is within the canal, and the sympathetic fibres or ganglia are not involved."

This explanation has not been accepted by some of my physiological friends as entirely satisfactory, but their views are based mostly upon theoretical grounds.

A most interesting case has just come under my care, and appears to me to give great force to the views expressed. The following is the clinical history:

Thomas McC—, stevedore, aged fifty, was brought into the Pennsylvania Hospital February 1, 1882, about 10 o'clock, A. M. A short time before, he fell through the hatchway of a vessel upon the shaft alley, a distance of about ten feet below, and struck with great force upon the back of his head and neck. He was a large and splendidly muscular man, with no superfluous fat.

He was perfectly conscious, but absolutely paralyzed, both as to sensation and motion, from the line of the second rib downwards. The skin was sensitive over the deltoids, but below this there was no feeling or power of motion in the upper extremity.

He complained of no pain, but had a terrible sense of constriction, and a feeling of want of breath. The breathing was entirely diaphragmatic. The thoracic muscles could not be seen in action anywhere. There was priapism, which, for most of the time, was complete, and always marked. The pupils were contracted and irresponsive to light. Electro-muscular contractility was not impaired. There was scrotal and plantar reflex after recovery from shock; the temperature in the axilla rapidly rose from 97° to 105°, which point it reached on the fourth day, when the patient died. The respirations ranged from 20 on the first day to 32 on the last. On this day they were very labored, as was indicated by the movements of the larynx. The chest was greatly expanded, and there was a deep sulcus across the superior abdominal regions; below this there was meteorism. The pulse-range was from 50 on the first day to 98 on the last, and always feeble. The urine was drawn off with a catheter; it was of 1035 sp. gr., and loaded with phosphates. The surface temperature was, on the second day: Scrotum, 97½°; inside thigh, 96°; third day, scrotum and thigh, 96½°; fourth day, inside thigh, 98½°. The dyspnoea increased, and the patient became unconscious before death. The diagnosis was based on the symptoms only, for it was not deemed prudent to raise the patient from his bed or to turn him. It was diagnosed that there was a fracture of the cervical vertebra from the third to the fifth, with laceration of the cord, but not completely involving the phrenic nerves, for by these alone the patient breathed. The opinion was ventured, also, that the cervical portion of the sympathetic nerve was involved in the injury.

*Autopsy.*—The chest and abdomen were first opened. The lungs were somewhat adherent from old pleurisy, but they were engorged with blood. The right heart was enormously distended. The neck was now carefully dissected. On raising the great vessels, a line of clotted blood from laceration laid along the deep muscles. The superior cervical ganglion of the sympathetic on the right side was carefully sought for above the injury. It was apparently intact, but upon tracing the nerve downwards, it passed into a line of clot, and the third cervical ganglion was imbedded in a mass of blood, and a firm clot was pressing upon it. The left side showed, also, blood clot in the same position, but the nerve was not dissected. Dr. Morris Longstreth, who was present and assisted at the autopsy, has carefully preserved the specimen. The clots about the nerve had nothing to do with the lesions of the column and cord which were now sought for.

In fact, the absence of hæmorrhage about the injuries which were here found was a matter noticed by all who were present. There was a complete and clean fracture through the intervertebral substance between the third and fourth cervical vertebrae, involving the bodies of both as though the bones had been subjected to rough grinding. There was also separation between the fourth and fifth and fifth and sixth vertebrae. Directly opposite the main lesions, those of the third and fourth vertebrae, the cord was lacerated and a small hæmorrhage had taken place within it. The patient

appeared to have sustained a tremendous blow with doubling backwards on the line of the main injury, while the other vertebral connections and the surrounding structures were stretched and lacerated.

Dr. Morris Longstreth made some thin sections of the superior cervical ganglion. Hæmorrhage into its substance is very distinct under the microscope. The inferior ganglion was subjected to grosser lesions, for hæmorrhages in and about it are visible to the naked eye. It is the intention of the Doctor to examine and report upon these lesions more fully.

Besides Dr. Longstreth, Dr. John H. Packard, Dr. Jos. M. Fox, the resident physicians of the hospital, and several other medical gentlemen, were present at the autopsy.

In this case do not the enormously distended heart, the engorged lungs, the temperature record, and the priapism, all point to involvement of the vaso-motor system in the injury, and consequent interference with its inhibitory or regulating functions?

The most skillful vivisectionist could not have made a cleaner and more limited break of the spine and laceration of the cord than were done by the accident in this case. There was no pressure on the cord from without by bone fragments or hæmorrhage. The cervical nerves arising above the lesion gave to the phrenics their partial power, and through them and the diaphragm the man lived by actually working for his breath. *He never slept!* Towards the last coma came on rapidly followed by death.

The unimpaired electro-muscular contractility was due, I take it, to the fact of the muscles being perfectly sound in structure and the response was *sui generis* and independent of nerve supply, for in these cases, when the patient lives long enough and the muscles become altered by degenerations, there is little or no response to the electric current. Patellar and ankle reflex were absent, probably because the deep muscular nerves were irresponsive to the rough shock necessary to produce them; while surface reflex through the cord below the lesion was present and is readily understood.

These facts, together with the lesions of the sympathetic nerve and the consequent vaso-motor phenomena, make the case, it appears to me, one of the most interesting and instructive ones on record.

## ORIGINAL ARTICLES.

### A REPORT OF SOME CASES OF GLAUCOMA

IN WHICH AN IRIDECTOMY ON ONE EYE SEEMED TO PRECIPITATE AN ATTACK OF ACUTE GLAUCOMA IN THE OTHER.

BY C. R. AGNEW, M.D., AND D. WEBSTER, M.D.,  
OF NEW YORK.

OUR remarks upon these cases will be brief, as the cases are more interesting and instructive in themselves than anything we can say about them.

That an operation for the relief of acute glaucoma in one eye may quickly be followed by an attack of the same disease in the other, is no new doctrine to the ophthalmic surgeon. Von Graefe, who first performed the operation of iridectomy for the cure of glaucoma, also first observed and described the complication of which we speak. In his experience, the fellow-eye was attacked within fourteen days after the operation on the first in about thirty per cent. of the cases in which the operation was done for primary inflammatory glaucoma. The cases here related will show that it sometimes occurs after an operation for

chronic glaucoma, or for painful glaucoma absolutum. In fact, we do not think it safe to operate upon one eye for glaucoma of any kind without previously warning the patient that the second eye may be attacked within a few days, or even a few hours, after the operation. The patient invariably attributes the attack in the second eye to the operation upon the first, and, if not informed beforehand of the possibility of its occurrence, will be very likely to blame the surgeon for withholding his knowledge of such liability.

We think it is now very generally conceded that the attack of acute glaucoma excited in the fellow-eye, in such cases, is not a true sympathetic inflammation. It is believed rather that anxiety of mind, the excitement attendant upon an operation, the hyperæmia of the eyeball, caused by the administration of ether, and the shutting up of the eye with a bandage, are sufficient to awaken an acute attack in an eye already predisposed to glaucoma. And we think it is fair to infer that an eye which has shown no glaucomatous symptoms, subjective or objective, is *predisposed* to the disease if the fellow-eye is so affected.

In the majority of cases where the second eye is so attacked, we have found it necessary to operate upon it with as little delay as possible, and in all these cases we have obtained satisfactory final results. In some cases, however, it will yield to antiphlogistic treatment. We remember a case operated upon at the Manhattan Eye and Ear Hospital, but not reported here, where the attack in the fellow-eye passed off in a day or two, under the use of eserine, and did not return. In the ninth case here reported, the attack in the second eye passed off without an operation, under the use of opiates and iced cloths.

In all our later operations for glaucoma, we have left the fellow-eye unbandaged, and in none of the cases where we pursued this method, have any symptoms of irritation in the second eye shown themselves. We have not followed this method in a sufficient number of cases to base an opinion upon, but we hope to find that the immediate attack upon the second eye will always thus be avoided.

Our *first* case is that of a married woman, forty years old, who came to us with a history of chronic glaucoma, in November, 1872. She had then R. V. =  $\frac{2}{30}$ , L. V. =  $\frac{2}{30}$ . Hm.  $\frac{1}{30}$  each. T + ? both. Ophth.: Vitreous hazy, retinal veins enlarged and pulsating, disk reddened, retina looking oedematous, and a commencing crescent of staphyloma posticum in both.

She was seen again in February following, when all the symptoms were improved, and she read  $\frac{2}{30}$  with each eye with +  $\frac{1}{2}$ .

On February 20, however, we were sent for, and found the left eye in a condition of acute glaucoma, with sight reduced to perception of light. Ether was at once administered, and a broad, peripheral iridectomy done upwards by Dr. Agnew. On the evening of the next day the fellow-eye was found hard, red, and painful. The pupil was dilated and the vision reduced to perception of light. Iced cloths were applied and anodynes administered, but as the eye was no better the next morning we again gave ether, and Dr. Agnew performed an iridectomy on the second eye. From this time the recovery was good, and some months later the lady read fine print with each eye.

Our *second* case is that of a carpenter and builder, forty-eight years old, who gave the following striking history: About six months before consulting us he went to church one Sunday morning, seeing as well as ever, and was led out by his sister "stone blind." He could see a little by the time he got home. He does not remember having had any pain at the time. Since that time his left eye has become bloodshot and painful, with obscurity of vision about once a month, the vision remaining a little worse after each attack, until *now* it is reduced to perception of light. Eyeball hard, but not red or painful; pupil hazy, not dilated, but sluggish. Ophthalmoscope revealed only a slight reddish reflex from fundus, because of diffuse opacity of the vitreous and translucency of the cornea. R. V. =  $\frac{3}{8}$ : $\frac{3}{8}$  with +  $\frac{1}{4}$ .

October 26. Iridectomy by Dr. Agnew on left eye.

The next morning there was a slight dull pain in the right eye. Two days after the operation the right eye was red, pupil dilated, and vision reduced to counting fingers at four feet.

On the third day after the operation on the left eye, an examination having shown that the acute glaucoma in the right was growing worse in spite of treatment, we placed the patient again under ether, and Dr. Agnew performed an iridectomy upon the right eye. The recovery was smooth.

In March, 1875, five months after the operations, R. V. =  $\frac{3}{8}$  with +  $\frac{1}{4}$  c. ax. 10°, and visual field normal. L. V. = counts fingers in a small field.

Our *third* case is that of a female nurse, sixty-eight years of age, with painful glaucoma absolutum of right eye, and chronic glaucoma of left. L. V. =  $\frac{3}{8}$ ; no improvement with glasses. Visual field less than one-fourth its normal size. Tension normal, but glaucomatous excavation of disk.

January 12, 1874. Iridectomy upon right eye, by Dr. Agnew, for relief of pain.

January 14. The fellow-eye has been injected, lachrymose and somewhat painful since the morning following the operation. Anterior chamber shallow, tension increased, vision cloudy. Dr. Agnew now performed an iridectomy on the second eye, and both recovered kindly.

In March, 1878, more than four years after the operations, the patient returned, complaining that the use of the left eye, which had now vision  $\frac{3}{8}$ , produced pain in the blind right eye, which was then enucleated with entire relief to the patient.

Our *fourth* case is that of a carpenter, forty-eight years old, upon whose left eye iridectomy was performed by Dr. Agnew, for glaucoma simplex, on October 1, 1873. The same night he complained of sharp, shooting pains in the eye operated upon, which were relieved by a hypodermic injection.

On the *third* day after the operation, the *right* eye became red, painful, and lachrymose, and vision was reduced from  $\frac{3}{8}$  to counting fingers at two feet. Two leeches were applied to the temple, and atropia and iced cloths used. More or less improvement resulted, and the eye was kept under observation, and treated according to indications until the *eighth* day after the operation on its fellow, when the sight being still reduced to ability to count fingers, it was thought best to perform an iridectomy upon it also, which was accordingly done by Dr. Agnew. Both eyes recovered well after this.

Our *fifth* case was that of a married woman, fifty-two years old, whose father had been blind for fifteen years with glaucoma absolutum. She had a history of chronic glaucoma extending back over several years, and latterly of acute exacerbations in the right eye.

R. V. =  $\frac{3}{8}$ : $\frac{3}{8}$ : no impt. with gl.

L. V. =  $\frac{3}{8}$ : $\frac{3}{8}$  with +  $\frac{1}{4}$ .

Ophthalmoscope showed left fundus and media normal; right, dotted and diffuse opacity of the cornea, more dense towards the centre, and shading off towards the periphery. An iridectomy was done upon the right eye by Dr. Webster, June 27, 1877, in the afternoon. The next morning, on removing the bandage, the left eye was found to be slightly injected, the iris bulging forwards, the pupil slightly enlarged and sluggish, and the vision misty; iced cloths were applied.

In the evening, the left eye was found a little worse in every way, and beginning to be painful. Codeia was ordered to relieve the pain.

In the morning of the *second* day after the operation, the eye seemed to be better. In the evening the patient counted fingers with each eye by candle-light, but more distinctly with the operated eye than the other.

The next morning, the *third* day after the operation, there was no pain in the eye, but as the sight remained very much blurred, and all the symptoms of acute glaucoma persisted, we made arrangements to operate in the afternoon.

When we went prepared to operate at 4 P.M., we found the lady suffering from the most intense pain in the second eye. This pain had come on suddenly, about an hour before, and the patient described the feeling as that of a sharp knife being constantly thrust around the back of the eyeball. She declared she would rather have the eye taken out than to submit to the excruciating pain an hour longer. Upon separating the lids a large quantity of hot tears burst forth, the eyeball was found intensely congested, and there were many ecchymotic spots in or beneath the ocular conjunctiva.

The cornea was so cloudy that it was with some difficulty that the dilated pupil could be seen; the hardness of the eyeball was very marked, and the vision was only perception of light.

An iridectomy was at once done, under ether, by Dr. Webster. Six weeks after the operations

R. V. =  $\frac{3}{8}$  with +  $\frac{1}{4}$  c. ax. 180°.

L. V. =  $\frac{3}{8}$  with +  $\frac{1}{4}$  c. ax. 180°.

She has had no recurrence of the disease, more than four years having elapsed since the iridectomies.

Our *sixth* case is that of a married woman, sixty-three years of age, the subject of goitre, with a history of chronic glaucoma of the left eye, with acute exacerbations.

R. V. =  $\frac{3}{8}$ : $\frac{3}{8}$  with +  $\frac{1}{4}$ . Field and ophthalmoscopic appearances normal.

L. V. =  $\frac{3}{8}$ ; no improvement with glasses. Field contracted to a diameter of *one inch* at a distance of *one foot*. Pupil enlarged and fixed; tension +; ciliary veins engorged; shallow anterior chamber.

Ophthalmoscope showed slight diffuse corneal opacity, extensive cupping of disk, with exposure of cribriform fascia.

A drop of a four-grain solution of eserine was put into the left eye, which caused a twitching sensation in and about the eye.

The next day the patient reported that the eserine caused pain for an hour or two, but did not improve the sight, and as we found the condition of the eye the same, we placed her under ether and Dr. Agnew performed an iridectomy upon the eye.

When the eyes were opened on the morning of the *second* day after the operation, neuralgia over the right eye was just commencing. The pain increased in severity, and kept her awake all night, although she took a grain of codeia. In the morning of the *third* day, eyeball red, pupil dilated and fixed, anterior chamber shallow, tension increased, corneal haziness, vision of fingers at three feet.

An iridectomy was done upon the eye, at 2 P.M., by Dr. Agnew, after which the patient slowly recovered. Six weeks later:



R. V. =  $\frac{20}{60}$  with +  $\frac{1}{2}$ c. ax.  $180^{\circ}$ c. —  $\frac{1}{2}$ c. ax.  $90^{\circ}$ .

L. V. =  $\frac{10}{60}$ ; no improvement with glasses.

Our seventh case is that of a maiden lady, forty-five years old, who had suffered from weak eyes since measles at fifteen. For five years she had been subject to attacks of neuralgia of the head, occasionally shooting into the right eye. For the last three years the sight of the right had been impaired, but the patient believed she could read with it up to the day before we saw her. She had never tried it separately, however. The eye grew red and painful the day before she consulted us, and she was kept awake all night by the pain. When we first saw her the lids of her right eye were slightly swollen, the eyeball red and watery, the pupil dilated and fixed, the anterior chamber shallow, the tension increased.

R. V. =  $\frac{10}{60}$ ;  $\frac{20}{60}$  with +  $\frac{1}{2}$ c; field contracted.

L. V. =  $\frac{20}{60}$ ;  $\frac{20}{60}$  with +  $\frac{1}{2}$ c; field normal.

Ophthalmoscope showed in the left eye no lesion; in the right, a diffuse opacity of cornea, which interferes with examination; but a cupped disk, surrounded by a broad whitish ring, can be discerned.

The right eye being very painful, we dropped into it some of a four-grain solution of eserine. An hour and a half later the eye felt much better. The patient was furnished with some of the same solution to drop into the eye twice daily.

Two days later, the patient reported that the eserine so relieved the pain that she had slept well both nights, but the vision was unimproved.

The next day, as the pupil remained dilated and immovable, and the vision unimproved, the patient was placed under ether, and an iridectomy done upon the eye by Dr. Agnew. On the sixteenth day after the operation, it was noted that the eye had steadily improved, and that the sight had risen to  $\frac{20}{60}$  with +  $\frac{1}{2}$ c. Seven weeks after the operation, as the fellow-eye was red and irritable, atropia was dropped into it, and adhesions of the papillary border of the iris to the anterior capsule of the lens were discovered. Atropia was then used, *coup sur coup*. Two days later the adhesions had been broken up, and the eye was feeling comfortable. The atropia was stopped.

Ten days later, or at about the end of the ninth week after the operation on the first eye, the patient came to us with well-pronounced acute glaucoma in the other. The first eye was looking well. Ether was now given, and an iridectomy done upon the second eye by Dr. Agnew. Ten days later R. V. =  $\frac{20}{60}$  with +  $\frac{1}{2}$ c, L. V.  $\frac{20}{60}$  with +  $\frac{1}{2}$ c. The eyes gave no further trouble.

Our eighth case is that of a married woman, twenty-six years old, who one evening had pain in her right eye before going to bed. There was also some obscuration of vision, but the next morning the pain had passed off, and the sight seemed as good as ever.

Similar attacks occurred several times, when, becoming alarmed, she consulted an ophthalmic surgeon, who performed an iridectomy upwards upon the affected eye.

Inflammation, with loss of vision, set in on the second day, and on the third acute glaucoma attacked the fellow-eye. She presented herself at the Manhattan Eye and Ear Hospital one week later, when her condition was as follows:

Right eye red and painful; lens opaque and swollen; no anterior chamber; tension much increased; vision reduced to perception of light. Left eye red and painful; tension increased; very shallow anterior chamber; pupil so widely dilated that only a very narrow rim of iris could be seen, this rim being a little broader below than elsewhere. Opacity of the media prevented the fundus from being seen. Vision of fingers at nine feet.

Ether was given, and, Dr. Agnew operating, a circular disk of the right cornea was removed by means of

Bowman's smaller trephine. A portion of the swollen-lens matter escaped through the opening.

An iridectomy downwards was done on the left eye at the same sitting, and, in order to keep up the drainage from the eye for a longer period, a small piece of cornea was snipped away with the scissors at one extremity of the wound. Both eyes recovered kindly.

Eleven months later it was noted: Right pupil closed by a dense membrane, perception of light, but very imperfect projection.

L. V. =  $\frac{20}{60}$  with +  $\frac{1}{2}$ s. c. +  $\frac{1}{2}$ c. ax.  $180^{\circ}$ . No cupping of optic disk.

The patient was examined two years later and the condition was found unchanged.

Our ninth and last case is that of a woman, forty-five years old, who, upon consulting us in September, 1876, stated that she had suffered from very severe pain over her right eye for six weeks. The pain had generally been worse at night, and had grown much worse in the last two weeks.

Eye red, vision of fingers at one foot, pupil widely dilated, anterior chamber shallow, tension increased.

Ophthalmoscope showed only a faint reflex from the fundus. L. V. =  $\frac{20}{60}$ ; sufficient old corneal opacity to account for the lowering of vision.

September 8. Iridectomy upon right eye by Dr. Webster; slight hæmorrhage into pupil. No atropia used.

The next morning the blood had been absorbed from the anterior chamber, which was again filled with aqueous humor. There had been no pain since the operation.

In the evening the fellow-eye became red and watery and slightly painful. The pupil was moderately dilated and sluggish, and the vision was blurred.

An opiate was given, and iced cloths were applied through the night, except during sleep.

The next morning the glaucomatous symptoms had entirely disappeared, and the vision was again  $\frac{20}{60}$ .

Two years later we saw the patient again. Both eyes had remained well.

R. V. =  $\frac{20}{60}$  with —  $\frac{1}{2}$ c.

L. V. =  $\frac{20}{60}$ ; E.

#### A CONTRIBUTION OF FACTS

BEARING ON THE QUESTION OF THE DETERIORATION OF HUMANIZED VACCINE VIRUS.

BY DANIEL B. D. BEAVER, M.D.,  
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THE following account of an outbreak of variola is presented as containing facts of general interest to the profession at this time, when so much is being said about the relative merits of humanized and bovine virus.

The circumstances attending the appearance and extension of the disease are unique in many features. It occurred in a rural district (among a class of people whose statements are more reliable than those of the class of persons who find their way into the small-pox hospitals of the large cities), where every inhabitant is personally known to the physician, and consequently every case could be traced to its origin, and when, furthermore, the manner of primary vaccination in nearly every case was known.

The facts were communicated to me by Dr. Thomas T. Zerby, of Sheafferstown, Lebanon Co., where the disease prevailed during the winter and early spring of 1875, and briefly mentioned by me in the report of the Medical Society of Berks County to the Medical Society of Pennsylvania. But since then



additional knowledge of the matter has been furnished, which leads me to record it in this journal.

The disease appeared first in a family living about three miles from Sheafferstown. The manner in which these people contracted the disease could not be discovered. They were attended by one of the village physicians, who during their illness was in the habit of taking a friend, Mr. F., with him on his drives. In the course of some days Mr. F. was taken sick suddenly with fever, delirium, unconsciousness, ecchymoses, etc., and died the second day of his illness. His death was supposed to have been caused by some obscure blood disease, and the corpse was kept the usual time, and during the funeral service taken into the church.

The funeral was largely attended, the number of persons there having been estimated at twelve hundred. Within a week after the burial of this man new cases of small-pox appeared in and around Sheafferstown, and the disease spread rapidly until the number of cases reached two hundred. The infection in every case was traced either to Mr. F.'s sick-room or his funeral, the latter, of course, being the origin of nearly all of them, since the duration of his illness was too short to admit of many persons visiting him.

Prior to his illness and burial there was no case of the disease in the vicinity of Sheafferstown, except in the family above mentioned.

Of the two hundred persons who became affected with the disease, one hundred and ninety-four bore vaccination marks, and six were known not to have been vaccinated. The latter were four small children, and two adults; the children all died, and the adults recovered, making the death-rate  $66\frac{2}{3}$  per cent. Of the post-vaccinal cases only five succumbed to the disease, which left the mortality among them at  $2\frac{1}{2}$  per cent.

These cases included persons of all ages, many of whom had not been vaccinated for many years, since revaccination is seldom practised in the rural districts of this section. Even during the prevalence of small-pox, comparatively few persons resort to it. For the primary operation, as well as for revaccination, nothing but the crust of humanized virus has been used for many years. The children are vaccinated during the months of May and June, because at that time of the year physicians have usually least to do, and the children are in better health than during the heat of summer, and exposed less to the changes and inclemency of the weather which give rise to inflammatory affections in fall and winter. It is then necessary for physicians to preserve virus from one spring-time to another. This is done by selecting good scabs, covering them with beeswax, and immersing them in glycerin or some other preservative fluid until needed. Thus one year's virus furnishes the supply with which to begin the work of the next.

In this way vaccination has been performed from year to year during the whole professional life of many of our oldest practitioners. The common rule of practice is, to vaccinate, and on the sixth to the ninth day visit the patient to ascertain whether it "has taken." If it has not been successful, the

child is revaccinated, and, if this fails, the matter is postponed to some future and more favorable time, and often forgotten by both physician and parents. It must necessarily follow, then, that many of these persons are imperfectly vaccinated.

Notwithstanding all these deficiencies in the mode of vaccination, and the very severe exposure to the variolous poison of the persons who contracted the disease at Mr. F.'s funeral, the mortality compares favorably with that of other reports. It is even not much above that cited by one of the most active of recent assailants of humanized virus, Dr. Cameron, of Glasgow. He states that in eight thousand cases of post-vaccinal small-pox recorded in London, Edinburgh, France, and Copenhagen, between 1818 and 1830, the mortality was 1 per cent., and ascribes this favorable result to the fact that these reports cover a series of years which followed so closely upon Jenner's great discovery, that, as he says, "the lymph used was still comparatively fresh."<sup>1</sup> Whether this conclusion is correct or not, it is not within the province of this paper to question. It may be noticed, however, that Dr. Cameron does not take into account, in his argument, the well-established fact that methods of practice based upon recent discoveries are always more thoroughly executed during the years immediately succeeding their introduction, and the recognition of their value, than at a later period. Especially is this true of prophylactic measures, because at the beginning their usefulness is more apparent. Every one feels more keenly the importance of vaccinating carefully during the prevalence of small-pox than after its disappearance for some years; and so, undoubtedly, the same feeling led to a better performance of the operation soon after its introduction, when the widely-spread ravages of small-pox were yet fresh in the memory of many vaccinators, and when the new practice had yet to contend with inoculation, which was itself often as dangerous as the disease taken in the natural way. Now, after continuous observation, during more than three-fourths of a century of the efficacy of vaccination under every variety of conditions, we have become careless in the performance of it, which, although it may be taken as one of the best proofs of our confidence in its protective power, has probably been one of the principal causes of the increase of post-vaccinal small-pox. Furthermore, this remissness on the part of vaccinators has not only led to bad results in their own work, but has lowered the popular estimate of the importance of performing the operation thoroughly, until it has come to be regarded by many persons as so simple and trivial an affair that any one can execute it, and be the judge of its success, and given rise to the practice of self-vaccination. Instances of this kind, in which all the members of a family have been vaccinated by one of themselves, with virus obtained, perhaps, from the arm of a neighbor's child, are frequently observed in this city and the surrounding country. May it not be assumed, then, that there were many persons among the twelve hundred who attended Mr. F.'s funeral that were supposed to have been under the influence of the vaccine disease, who may

<sup>1</sup> London *Lancet*, American Reprint, p. 475, 1880.

have had marks on the arms, and yet were entirely unprotected against small-pox, simply because they had been carelessly vaccinated.

This being admitted, the deterioration of the protective influence of the humanized virus used in these one hundred and ninety-four cases, which was certainly very many removes from the cow, may be considered as almost *nil*, since the mortality of the whole number who bore vaccine marks was only  $2\frac{1}{2}$  per cent. If none had died, it would have been said that the protection was perfect.

It seems reasonable to conclude, then, that the cause of death in the  $2\frac{1}{2}$  per cent. of these cases was owing to imperfect performance of vaccination, carelessness in the selection of crusts, and neglect on the part of both physicians and patients in those instances in which a full development of the vaccine disease did not follow a first or second vaccination, rather than to any loss of vitality in the virus consequent upon its repeated passage through, and regeneration in, the human body.

#### REMARKS UPON SMALL-POX AND VACCINATION.

BY HARVEY L. BYRD, M.D.

PRESIDENT, AND PROFESSOR OF OBSTETRICS AND DISEASES OF WOMEN AND CHILDREN IN BALTIMORE MEDICAL COLLEGE, BALTIMORE, MD.

THE prevalence of small-pox in so many portions of our country during the autumn and winter of the current year, would seem to render almost anything that might be said, looking to its amelioration or prophylaxis, particularly interesting to the profession at the present time.

The March (1881) issue of the *Independent Practitioner* contained an exceedingly practical article on the mode of vaccinating, from the pen of Dr. Barton Dozier, an intelligent young physician of Ukiah City, California; and as I have practised his method in about two hundred cases, I am prepared to speak of it in terms of genuine commendation. He says: "I use *only fresh bovine virus*, and prefer that on ivory points to any other. I dip the point to be used in *cold water*, then lay it aside, in order that the virus coating may become well dissolved or softened. In the interim, I scarify with a dry ivory point, stroking just hard enough to scrape off as completely as possible, *without bringing blood*, the cuticle or scarf-skin, for a space of about a quarter of an inch square. I then rub the previously prepared point over this abrasion, until the virus is all off, and insist on the sleeve being kept up for at least five minutes *after* the operation is completed." Dr. Dozier further recommends that "a piece of adhesive plaster of sufficient size to cover the scarification" should be applied before the sleeve is drawn down. He tells us that he "got as a result *ninety-seven* per cent. of successful vaccinations."

My own success has equalled his since I have adopted his method, and such of my professional friends as have pursued the plan of *scraping* off the cuticle, have been highly pleased with the results. In fact, I think it capable of affording more satisfaction than any other method whatsoever.

My failures have been absolutely *nil*, where the

susceptibility to the action of vaccine virus had not been overcome by its previous introduction into the system, or an attack of variola, since I have adopted the course recommended above. I am fully satisfied, from long experience, that vaccination, when performed according to the usual methods, and with humanized virus, may be repeated successfully a number of times before the system is thoroughly *saturated* with it, or a point of complete immunity against an attack of variola is reached. I use the word *saturated* for the lack of a more expressive one, as I am entirely satisfied that when a certain point is reached, the system will receive no more impression from the virus; and then the protection against small-pox is perfect and absolute for all time to come. How often it may be necessary—if necessary at all—to repeat the process of vaccination by *scraping* off the epidermis and applying *bovine virus*, fresh from the cow, I am not prepared to say, as my experience in this way has been too brief. But when the point of saturation is reached, the system is fully protected against the invasion of variola.

Another interesting fact mentioned by writers on vaccination, has frequently been verified by my own observations, and the truth of which should be as widely disseminated as possible, viz., that vaccinia may be successfully introduced, and its protective power secured even in the very presence of variola, so to speak; and its influence against the latter relied upon implicitly. Thus, by way of illustration, a case of small-pox may occur, or is introduced into an unprotected, unvaccinated family, but should successful vaccination with bovine virus be immediately, or even within a few hours, performed upon the other members of the family or household, such persons will be protected entirely from the variola under which the patient labored. This fact I have verified in several instances, and would respectfully and confidently urge its adoption as a safe and reliable prophylactic against the small-pox germs or contagion, even after pustules have been fully developed, and free intercourse has taken place for several hours between the patient and unvaccinated persons. The more rapid saturation of the system by the vaccine virus, and consequently earlier development of the several phases, or stages, of that disease, than that of the variola germs, should always cause it to be resorted to with reliable bovine virus, whenever an unvaccinated person is brought into contact with one suffering with small-pox. I favor repeating vaccination from time to time, until it ceases to affect the system, and when this point is reached, I feel warranted in saying that the protection against small-pox is absolutely perfect; the dicta and illogical statements of the opponents of Jenner's immortal boon to the human race to the contrary notwithstanding.

#### ETHIDENE POISONING.

BY EDWARD T. REICHERT, M.D.,

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ETHIDENE, or what is more technically expressed as ethidene bichloride, has recently come into vogue

across the water, and has been used to a considerable extent since the researches of the British Medical Association Committee were published, with results in some instances, as might have been anticipated, of the most painful nature.

In a recent article<sup>1</sup> I protested against the indiscriminate use of chlorinated, brominated, and iodated anæsthetics, and showed that among the most dangerous of all properties possessed by anæsthetics in general were decided depressing effects on the heart, and their liability to act at times altogether out of proportion to the dose, and that both these properties were possessed to a very notable degree by the compounds containing either of the halogens as constituents. Special reference was made to the ethidene bichloride, and evidence given which decidedly indicated that this substance markedly depressed the heart, and moreover, that a death had already occurred from its use. Since the publication of this article, I have gathered more evidence to substantiate these statements.

A record of about three thousand administrations with a list of three deaths, and four cases in which the most alarming symptoms ensued, imperatively cries a halt to this sort of experimentation, and drives the very chloroform death-list to shame; and at the present rate, the size of the death-list from chloroform in comparison with that of ethidene will be as insignificant in its proportions as the ether death-list is to chloroform. Therefore, in order that the profession in this country may fully appreciate the grave perils attending the use of this new anæsthetic, I have appended a summary of the above cases:

CASE I.<sup>2</sup>—A lady, about sixty, had lost blood from a vascular growth of the urethra. Within the first two minutes of inhaling I felt the pulse become small, and removed the inhaler at once. She continued breathing regularly fifteen or twenty times, then the breathing intermitted; the pulse was now imperceptible. I used artificial respiration. She recovered in two minutes; the pulse remained feeble; but I kept her insensible by occasionally giving the bichloride during the operation.

CASE II.<sup>3</sup>—A gentleman, aged thirty, was about being operated on for fistula. I gave nitrous oxide till he was stertorous, adding ethidene carefully. The pulse becoming small, I allowed fresh air for three inspirations. He then became delirious, and struggled, passing quickly into a state of opisthotonos. I allowed air till he had so far recovered as to speak, and then renewed the ethidene. As he became stertorous, I noticed the pulse became intermittent and stopped. I removed the inhaler; the breathing continued firm and regular. In about twenty seconds the pulse returned; the patient awoke in five minutes in glorious spirits, though covered with perspiration.

CASE III.<sup>3</sup>—An old gentleman had lost blood for a long time from piles. At first I had difficulty in making him breathe regularly. At last he commenced panting very quickly. As he began to snore, I noticed the pulse began to fail, and it stopped. I drew his head to the edge of the bed, allowed it to fall back, and drew forward the tongue, with my long finger passed into the pharynx. Artificial respiration was carried on for two minutes, when he recovered. He was put again in position, and I continued to keep him insensible by ethi-

dene during the operation. In five minutes after it was over, he expressed his satisfaction in the liveliest manner.

CASE IV.<sup>1</sup>—A man was about to have an operation performed for hæmorrhoids. He was pale from a loss of blood which had been going on for six months. The heart sounds were normal. Being placed on the right side, with his knees drawn up, bichloride of ethidene was administered with the gas, and ether inhaled. He did not respire regularly at first, but he never coughed, and was not observed to swallow. After twelve or fourteen respirations he talked and commenced to struggle. The face-piece was then removed for a little distance from the face, to allow him to breathe some fresh air. The struggling and convulsive movement which followed interfered with the precise observations of the state of the pulse; when these had ceased there was hardly any pulse, and it soon disappeared, but he continued to breathe ten or twelve times in a satisfactory manner, and then a pause was noticed between the respirations. His head was immediately extended backwards and downwards over the edge of the bed, and the tongue drawn forward, whilst the feet were raised, and artificial respiration commenced. This, in about a minute, restored him so far as to cause him to try to speak in an indistinct manner, as a patient often does just before waking up. The pulse became distinct, all danger seemed past, and he was again placed in position for operating. In doing this, he seemed to raise his head voluntarily, and some more of the anæsthetic was about to be given, when the pulse was again noticed to be failing. Without loss of time, the artificial respiration was recommenced as before, and, during the first three minutes, there were several natural gasping inspirations, but the pulse was never felt again.

*Autopsy.*—Heart large and flabby, with the usual amount of fat on it; heart substance on section had the ordinary appearance of fatty degeneration. The lungs were a little emphysematous and congested posteriorly, but fairly crepitant. Microscopical examination of the apex of the heart, including the extremities of both ventricles, was found to have undergone extensive fatty degeneration.

CASE V.<sup>2</sup>—A man, aged forty-five, was suffering from effusion into the right pleura cavity, and, after having determined that he was suffering from pyæmia, it was decided to give ethidene. During its administration the patient struggled a good deal, but nothing alarming occurred until he was "under," when it became necessary to turn him over on the left side and make an incision. He had not been in this position over two seconds when the pulse and respiration were found to be failing, and the pupils were noted to be widely dilated. He was then immediately turned on his back, artificial respiration commenced, and stimulants freely applied to the heart; first, by slapping the cardiac region with the hand, and afterwards, by a needle and galvanic current, but the only result was to get one long inspiration.

*Autopsy.*—The right auricle and ventricle contained much dark fluid blood; the tricuspid valve admitted four fingers. The left ventricle was empty. Microscopical examination of the heart showed no fatty degeneration. Other points of the autopsy are not of interest in this connection.

CASE VI.<sup>3</sup>—A boy, aged nine, was placed on the operating-table for operation of peritomy. Ethidene was given, and he was brought under its influence quietly and quickly. He then vomited freely, and more ethidene was given, but it was observed that his pulse

<sup>1</sup> Am. Journ. Med. Sciences, July, 1881.

<sup>2</sup> Clover, Brit. Med. Journ., 1880, I., p. 797.

<sup>1</sup> Clover, Brit. Med. Journ., 1880, I., p. 797.

<sup>2</sup> Mouillot, Brit. Med. Journ., 1881, I., p. 385.

<sup>3</sup> Everard, Brit. Med. Journ., 1881, I., p. 431.



became weak and fluttering, but rapidly recovering on stopping the ethidene. On resuming the ethidene anaesthesia was induced in about twenty seconds, and the operation commenced. It was very nearly completed when the pulse became almost imperceptible, and ceased entirely, together with the respiration; the face became cyanotic and congested, and the pupils widely dilated. The head was promptly but deliberately lowered, the chin drawn away from the sternum, and complete inversion of the body practised by suspending the child by the feet. He showed no signs of life for two minutes, when a long, deep gasp ensued, followed by renewed and deeper lividity of the face. In another minute another deep gasp ensued, and finally, by interrupted pressure over the cardiac region and lower ribs, the respiration and circulation were completely established.

CASE VII.—This case, in which death ensued, occurred in Berlin, and was reported by Kappeler in Part XX. of Billroth and Luecke's *Surgery*. I am, unfortunately, unable to give the details.

It will be noticed in reviewing the above cases that in every one of them, except the last, in which no details are given, there was a manifest and unmistakable failure of the heart, and that both deaths were due to this cause. In each of the other four cases the pulse had entirely ceased, and it was only by the efforts of the attendants that the heart's action was restored. It is also a noticeable fact, that while the above experimenters appreciated the depressant action of the drug on the heart, yet in not a single instance is there any evidence to show that any of them were aware of the fact that amyl nitrite had proved so beneficial in chloroform poisoning and other states of cardiac depression, or that its use was even thought of in case of accident during their rash procedures. Ethidene was abandoned by Snow on account of its dangerousness, and every investigator since his time must have appreciated the truth of his belief. The experimental researches of the British Medical Association Committee and Reeve have clearly demonstrated a decided depressant action on the circulation, and notwithstanding the fact that the above Committee were lead to anticipate from the results of the experiments on animals that there was a danger of cardiac failure, they, after making six experiments on man, reached the unwarrantable conclusion, that the anaesthetic "presents all the advantages of ether, without any of its disadvantages." The Committee state that in these six experiments no failure of cardiac action was present, yet their own, and also Reeve's, experiments on animals, as well as the evidence of Liebrich, Steffen, Clover, Hodges, and Mouillot, who have used the anaesthetic on man, all testify to the contrary. Hodges even thinks that it more quickly and markedly depresses the heart than chloroform, but the balance of the evidence goes to prove that it is not so powerful in this respect, and that it is not so dangerous. This is in accord with my own views, and I think that if the above four cases, in which recovery occurred, had been under chloroform, the chances of resuscitation would have been few indeed.

Our knowledge of the physiological action of ethidene indicates that its action is identical with

that of the ethylene compounds, and having this fact in mind during a study of the latter preparation,<sup>1</sup> I considered it as the equivalent of ethylene bichloride, and stated that as an anaesthetic for general use the ethylene compound was undoubtedly superior to any yet introduced, with the exception of its isomer ethylidene chloride (ethidene bichloride) and ether, and was inferior to the latter in but the single point regarding the relative degree of safety, and that while it was probably equal to chloroform in strength, promptness, and permanency of effect, it was fully as much superior to it in a point of safety as it was inferior to ether, and that in case of accident following its use, the absence of the pulse at the wrist would not be so serious as under other circumstances. These deductions were the result of a careful study of direct experimental evidence, to which I will have to refer the reader, and I have had no sufficient reason since to alter my opinion, although the large percentage of deaths and accidents, as above given, makes it appear that ethidene is probably more dangerous than chloroform, but which cannot be so in view of our knowledge of its physiological action.

Ethidene is certainly a very dangerous anaesthetic, and should not be used except in cases where ether is contraindicated, and never without observing the precautions necessary in the use of chloroform, or without the amyl nitrite at hand.

## HOSPITAL NOTES.

### PHILADELPHIA HOSPITAL.

(Service of J. WILLIAM WHITE, M.D.)

#### APPARENT PROTECTION FROM SYPHILIS AFFORDED BY PHAGEDÆNA.

(Reported by W. A. EDWARDS, M.D.)

CASE I.—John F., æt. fifty, was admitted to the venereal wards of the Philadelphia Hospital, on November 16, 1881. He was then suffering from a sore, which had appeared about six weeks previously, on the left side of the inner surface of the prepuce just posterior to the corona; in the left groin he had a very irritable sloughing bubo, about four inches in diameter. The sore presented the following appearances.

It was slightly elevated above the surface, the edges were sloping, its floor was grayish, and in places was covered by false membrane or a scaly exfoliation; the secretion was scanty and serous. The induration was very characteristic, firm, and cartilaginous, circumscribed and movable upon the surrounding tissues; this feeling was persistent throughout the case. There was no pain or tenderness. In other words it was a typical indurated or "infecting" chancre. The bubo, as before mentioned, was situated in the left groin about midway between the anterior superior spinous process of the ilium and the symphysis pubis. It was an open sore, highly inflamed and painful, the gland tissue being entirely broken down; its interior was covered with a grayish diphtheritic deposit, and its edges were everted and undermined. It was extending rapidly. The man was placed at perfect rest, iodoform was applied to the chancre, and a poultice to the bubo; after a few days the bubo was packed with lint saturated with a weak solution of sulphate of copper, about 4

<sup>1</sup> Phila. Med. Times, May, 1881.

grs. to  $\frac{3}{4}$  v.; all sinuses were opened as soon as they formed; this was continued for a short time, but the bubo continued to spread with great rapidity. The acid nitrate of mercury and a solution of the potassium-tartrate of iron were successively applied, but it steadily grew larger, dissecting above and below Poupart's ligament, exposing the external abdominal ring and bringing into view the sheath of the femoral vessels the pulsation of which could plainly be seen on the floor of the ulcer. The actual cautery also failed to arrest the progress of the ulceration, and in spite of tonics, good food, free stimulation and careful attention, the patient died on January 7. No *post-mortem* examination was obtainable. During all the stay in hospital the patient was carefully watched with a view to the recognition of any symptoms of constitutional syphilis. Although the average time for their appearance was far exceeded, ninety-five days after the appearance of the sore having elapsed, there was no evidence whatever, either cutaneous or glandular, of systemic infection.

CASE II.—Hermann S., æt. thirty; was admitted December 2, 1881. He presented a sore just within the meatus, involving both its lips, and which had appeared four weeks previously and about twelve days after intercourse. In the right groin was a very well-marked phagedenic bubo, extending over a large area and causing much destruction of tissue; its sinuses had been slit up before admission, and it had just ceased to spread. In this case, also, the sore had all the well-known characteristics of the initial lesion of syphilis. The sore and bubo were packed with a solution of sulphate of copper, 4 grs. to  $\frac{3}{4}$  i., and are now perfectly healed.

This patient, who is still under observation, more than three months having elapsed since the appearance of the sore, has shown no symptoms of secondary syphilis; the epitrochlear and post-cervical glands are not enlarged or indurated; the tonsils and pharynx are normal; no eruption of any description has shown itself.

*Remarks.*—The chief interest attached to these cases, as pointed out by Dr. White to the class, was in their relation to the conflicting theories as to the manner in which syphilis infected the system at large, and as to the true character of the primary sore; whether it is a local manifestation, as is claimed on the one hand, or the first symptom of constitutional disease, as is asserted on the other.

Admitting that the diagnosis was correct, and that the sores were of the kind that in the vast majority of cases is followed by easily recognized secondary symptoms, the absence of those symptoms is inexplicable, if the chancre be considered an evidence of systemic involvement.

If, however, the chancre be merely the result of local cell proliferation, at the point of original inoculation, and if from that locality the poison gradually find its way into the general circulation through the lymphatics of the penis and the groin, it can readily be imagined that such enormous destruction of tissue and complete elimination of glandular and other structures, as take place in phagedæna, might afford an actual protection from constitutional disease. Ricord and Fournier have both remarked as a clinical fact that phagedæna of the chancre often prevents syphilis from affecting the ganglia. It may be, therefore, that phagedæna of the ganglia exerts a similar influence in the further spread of the disease.

This view would seem to be corroborated by the results of the treatment of chancre by excision obtained by Dr. White, during this term of service, and which will be published when a sufficient number have been observed to permit of generalization.

## MEDICAL PROGRESS.

ON A PECULIAR FORM OF GONORRHOÆAL ARTHRITIS.—Two varieties of gonorrhœal arthritis are usually described. In the first of these, the serous, the knee is most often affected, but the ligaments and other tissues round the joint escape. In the second form pain, swelling, and redness are all more marked. The connection of the latter variety with gonorrhœa has been doubted, and some authors have regarded it as the rarer form of the two. DUPLAY and BRUN have arrived at a contrary opinion. They state that the second variety occurs with equal frequency in both sexes, that there is no relation between the severity of the urethritis and that of the articular lesion, and that the wrist and elbow are the joints most often attacked. The knee, so often the seat of hydrops, is relatively but seldom affected by this severe form of arthritis. In general there is an incubative stage lasting some days, accompanied by painful tension in joints and muscles, which outwardly exhibit no change, and general depression. Then some particular joint becomes extremely painful and swollen. Pain, aggravated at night and rendered excruciating by pressure, is the first symptom. There is no intra-articular effusion, but extensive infiltration into the surrounding tissues is evidenced by tense and often widespread œdema of the cutaneous coverings. This condition may be mistaken for abscess, and incisions have been made, of course without evacuating pus. The pain on pressure over the line of the articulation, and, later on, crepitation during movement, help to distinguish this disease from phlegmon. The failure of salicylic acid in some cases excludes another source of error. The treatment consists solely in complete fixation of the joint by means of plaster-of-Paris bandages, the application of which is speedily followed by diminution of swelling and cessation of pain. After from four to six weeks the dressing may be removed and passive movements commenced. If, however, the plaster bandages be not applied till after the disease has existed some time, the joint, after their removal, will be found considerably stiffer, or even completely ankylosed. In some cases all treatment fails, and notwithstanding all that can be done, ankylosis results (Duplay and Brun, *Archiv Général de Med.*).—Riedel, in *Centralblatt für Chirurgie*.—*Edin. Med. Journ.*, Feb., 1882.

PHOSPHORUS AND ARSENIC POISONING.—An interesting series of researches on the pathological changes produced by phosphorus and arsenic poisoning have been communicated to the Société de Biologie by MM. CORNIL and BRAULT. The quantity of phosphorus was equivalent to that contained in four or five matches, or to five centigrammes of arsenious acid. Guinea-pigs to which this dose was given daily lived for four or five days, passing arsenious acid by the bowel and kidneys. The object of the investigation as regards the liver was to discover the seat of the initial lesions, and their mode of extension in the hepatic lobule. The normal liver cells, treated with osmic acid, are limited by a delicate membrane, which presents a double contour, and in the middle of the cavity is a nucleus, which is connected with the capsule by a delicate protoplasmic filament. Six hours after the ingestion of the first dose of phosphorus, the hepatic cells, which are situated at the periphery of the lobule, near the branches of the portal vein, are increased in size; their protoplasm is, as it were, liquefied, and contains albuminous granules and some fine fatty granules. This lesion gradually increases, but remains limited to the periphery of the lobule, until, at the end of twenty-four hours, the cells there are large and distended by albuminous and fatty

granules and globules, while those in the centre are almost unchanged, and two or three days after the commencement of the poisoning the dilated cells constitute a sort of reticulated zone in the periphery of the lobule, and the delicate septum which separates two adjacent cells may break down, so that their cavities become united. The biliary canals present no alteration, even catarrhal inflammation being absent. In arsenical poisoning the change in the liver presents an important difference. The fatty degeneration is less intense than in phosphorus poisoning; but, instead of being confined to the peripheral zone of cells, it is uniformly distributed throughout the lobule. The lesions in the lung produced by the two agents are identical, and consist in a primary fatty degeneration of the epithelial cells of the alveoli; they become large and prominent, filled with granules and driplets of fat, their nuclei being preserved. The cells ultimately become detached, and fall into the alveoli, where they are mingled with an exudation which contains many blood-corpuscles. The change is less constant in poisoning by phosphorus than by arsenic. The resulting exposure of the vessels seems to afford an explanation of the congestions and ecchymoses which are met with in the lung. Partial pulmonary lymphangitis was also found after death from arsenic. The lesions in the kidney are the same as those which have been before described. The epithelial cells of the convoluted tubes are swollen, and contain conspicuous fatty granules a few hours after the commencement of the intoxication. The cavity of the tubules also contains an exudation which is colored by osmic acid. In cases of longer duration the fatty globules are larger, and the epithelial cells, instead of being sharply separated, are reduced to a uniform mass, more or less filling the tubules. In other organs fatty degeneration of the endothelium of the vessels was also found.—*Lancet*, Feb. 4, 1882.

**THE OPTIC DISK IN INJURIES TO THE HEAD.**—Dr. A. A. FOUCHER draws the following conclusions from his essay on this subject:

1. In spite of the presence of a papillary stasis due to a traumatic lesion of the brain, acuteness of vision may remain unaffected; hence the importance of an ophthalmoscopic examination of all cerebral injuries, whether vision is affected or not.
2. This vascular stasis, when taken alone, cannot inform us as to the nature or gravity of the cerebral lesion, and its absence does not show the absence of grave lesions of the cerebrum.
3. The only conclusion to be drawn from its presence is that the meningeal fluid is abnormally increased, and as a consequence intracranial pressure is increased, as shown by the compression of the sheath of the optic nerve.
4. This compression may be marked enough to interfere with the circulation of the nerve, and yet not severe enough to interrupt its functional activity.—*L'Union Méd. du Canada*, Jan., 1882.

**TREATMENT OF HYPERTROPHY OF THE TONSILS BY IGNIPUNCTURE.**—The removal of diseased tonsils by the help of the finger, ligature, cauterization by caustics or the actual cautery, are all methods which have fallen into disuse. In Europe, the bistoury only reckons a few partisans, amongst whom is one of great authority, M. de Saint-Germain. The treatment nowadays most in favor is excision, performed with the guillotine, which has given rise to very serious and often fatal hæmorrhages. It is now alleged that, with the thermo-cautery, this serious accident is no longer to be dreaded. M. KRISHABER, who has tried it during two years, and has collected more than forty cases (*Annales des Maladies de l'Oreille et du Larynx*, July, 1881), has never had

any accident after this treatment, and the results obtained have been lasting. It is likewise a novel application of a method which he has found perfectly successful for granulations of the larynx and pharynx. He proceeds as follows: The patient is placed—firmly, if a child—as if for laryngoscopic examination, in front of the operator, the mouth open, the tongue held back by a large spatula, the bottom of the throat well illuminated. M. Krishaber generally uses Paquelin's narrow-pointed thermo-cautery, heated to red-heat. When it is only required to modify the nutrition of the gland, he gives preference to Trouvé's polyscopic galvanocautery. The puncture of the gland, made as deeply as possible with the point of the instrument, should be repeated five or six times at each sitting. An interval of two or three days is left between the sittings, so as to allow the fall of the eschar, and to estimate the result. The operation is not at all painful, and pain, from burning, is rarely felt. Nothing need be administered after the operation, except, in some cases, a gargle of warm water, slightly carbolized.—*British Med. Journ.*, Jan. 28, 1882.

**INJECTIONS OF AN ALKALINE SOLUTION OF SALT INTO THE VESSELS IN ACUTE ANÆMIA.**—E. SCHWARZ (*Centbl. f. Gynäk.*, No. 20, 1881) holds that in cases of death from sudden hæmorrhage the fatal result is due not so much to the loss of blood-corpuscles as to the sudden change in the relation between the quantity of the blood and the capacity of the containing vessels. He refers to Goltz's work in *Virchow's Archiv*, No. 29, "On the Tonicity of the Vessels and its Relation to the Blood-Current." The writer, after taking two-thirds of their blood from dogs, injected a solution of salt, and from the good results he obtained, recommends this method in the human subject, not only for the collapse following profuse hæmorrhage, but also for that caused by severe wounds or prolonged operations, as in abdominal surgery, in which latter case he attributes the collapse to paralysis of the abdominal muscles. He recommends that 500 cc. should be the minimum amount injected, and says that three or four times this amount would do no harm. J. J. BISCHOFF, in the same Journal (No. 23), refers to the above paper, and gives his case as an instance of the value on the human subject of what had been used experimentally on animals. The patient suffered from post-partum hæmorrhage; the pulse scarcely perceptible, 156; respirations 42, had cold extremities, and was very restless. A 6 per cent. solution of salt, with a few drops of caustic potash and soda, was injected into the left radial artery in the direction of the blood-current, and the wounded vessel subsequently tied at both ends. A blunt vulcanite cannula, previously rendered aseptic, was employed. In the course of an hour 1250 fluid grammes was thrown in at a pressure of a column 58 to 60 cc. high. During the injection the patient revived, the pulse sank to 122, and she became quiet. Ultimately she recovered perfectly. It was noticed that meanwhile the hands regained their natural color and sensibility. The writer recommends similar treatment in cases where the blood-forming glands are healthy, but where there has been a sudden diminution in the quantity of blood, he suggests that even greater quantities of fluid might be injected, the injections to be continued from time to time till good results are obtained.—*Edin. Med. Journ.*, Jan., 1882.

**IMPURE CHLOROFORM.**—M. LUCAS-CHAMPIONNIÈRE has lately read a paper at the Paris Société de Chirurgie entitled "Etudes sur la Chloroforme." In it he states that Sédillot, who took great trouble to obtain pure chloroform, never had any accidents during its use. M. Perrin, in 1878, called the attention of the Academy



to the changes in chloroform. Wishing to remove hæmorrhoids with the *écraseur*, M. Lucas-Championnière anæsthetized his patient. All went well; but an hour afterwards a violent rigor supervened; in the evening, the patient was doing well. Four days afterwards, M. Championnière undertook the removal of a subcutaneous lipoma, near the scapula; the patient was chloroformed, and had a rigor like the one above mentioned. All the patients operated upon under chloroform at that time suffered from rigors. When the chloroform was changed, this symptom no longer appeared. In lying-in women, with certain kinds of chloroform it is impossible to produce hemianæsthesia. It becomes, then, a question, what are the substances which change the chloroform? On the 5th of July last, M. Lucas-Championnière performed the operation of radical cure of a very large right inguinal hernia, in a woman. Chloroform was administered; the respiration stopped. The patient was inverted, and artificial respiration applied; respiration returned at the end of four minutes. The chemist to the hospital declared that the chloroform was pure. Later on, in the same hospital, a youth, eighteen years of age, could not be anæsthetized; respiration stopped and necessitated inversion. Three days afterwards, with another kind of chloroform, the same patient could not be anæsthetized; but this time he became cold. On the next day, he was anæsthetized with another kind of chloroform. The first chloroform was examined by a distinguished Parisian chemist; the boiling-point was found to be modified; this chloroform was, therefore, impure, although the ordinary reagents testified to its purity. Permanganate of potash passes from red to green, if chloroform contain organic substances. With these reagents, M. Lucas-Championnière has found that throughout Paris there was no pure chloroform, although some specimens were almost pure.—*British Med. Journ.*, Jan. 14, 1882.

**A REMARKABLE GUN-SHOT WOUND OF THE ABDOMEN.**—Dr. J. A. RAFTER reports a remarkable case of gun-shot wound of the abdomen of a boy fifteen years old. The charge of a shot-gun, discharged about a foot from his body, entered the abdomen two and one half inches to the left and two inches below the umbilicus, making a terrible-looking opening, nine and one-half inches in circumference, tearing away the abdominal muscles to that extent, and leaving his intestines exposed and powder-burnt, and the intestinal liquid was plainly to be seen oozing from them at several points.

On turning him over, it was found that the charge had passed through him, shattering the ilium, and coming out at a slightly lower level than it had entered, and a trifle nearer the lateral aspect of the body. The posterior wound was the smaller; its lips were protruding and lacerated, due to the lodgment of gun-wadding, pieces of coat, vest, pantaloons, and shirt, which had been driven through him. When Dr. Rafter again examined the anterior wound, he found that considerable fecal gas and fluid had escaped from it.

The treatment ordered was nutritious diet, all the milk and brandy the patient's stomach would bear, and opium enough to check all peristaltic action, and keep him perfectly quiet. The case recovered.—*Med. Record*, Jan. 21, 1882.

**TREATMENT OF RUPTURE OF THE UTERUS BY DRAINAGE.**—In a recent number of the *Archiv für Gynäkologie*, Dr. FELSENREICH reports a successful case. To understand why a treatment succeeds, we must know why patients not treated die, and what unfavorable condition it is that the treatment removes. Now, the peritoneum has a great power of absorption, and, by exudation of organizable lymph, of encapsulating and shutting off foreign matter from the general peritoneal

cavity. There is no evidence that liquor amnii in the peritoneum is particularly hurtful, and large quantities of blood may be effused without causing peritonitis. There therefore seems no reason why the mere presence of the child, of blood, and of liquor amnii in the peritoneal cavity should not be tolerated. And as to drainage, the experience of ovariologists tend to show that under ordinary conditions the peritoneum will recover from the effects of surgical interference better without the drainage-tube than with it. The special service which drainage renders in rupture of the uterus is probably this: the peritoneum not merely has to dispose of some amount of blood and perhaps liquor amnii effused into it, and to repair a solution of its continuity; in addition, there is constantly being poured into it, through the rent, secretion from the interior of the uterus, as well as from the wound, and these secretions partly accumulate and decompose. It is these decomposing fluids which excite inflammation, and drainage, by keeping open a channel for their escape, and so preventing their stagnation, gives the patient a better chance of recovery.—*Med. Times and Gaz.*, Jan. 7, 1882.

**TREATMENT OF SNAKE-BITES.**—MR. VINCENT RICHARDS offers the following suggestions as to the treatment of snake-bites:

1. In the case of the bite being on a limb, a ligature should be at once applied above the bitten part, care being taken that it is sufficiently tight to prevent any blood being taken up into the general circulation from the distal end. Give a full dose of opium—40 minims of the tincture—or half a grain of morphia hypodermically.

2. Inject hypodermically into the bitten part a solution of permanganate of potash—4 grains to one drachm of water—and well press the parts with the fingers.

3. Open a vein below the bitten part, and wind round the limb an elastic bandage so as to exsanguinate the limb below the bitten part.

4. Cut through the bitten part and when dry apply pulverized permanganate of potash and then loosen the ligature. In the case of a person being bitten on the trunk, any treatment however prompt may be useless. Nevertheless it would be advisable to well inject the part with permanganate of potash, giving a full dose of opium as before recommended.

It may not be generally known to the members of the profession that a *poisonous* bite may be easily ascertained by cutting through the punctures into the areolar issue beneath, when if a red-currant jelly-like appearance is observable the bite is poisonous. The merit of pointing out the diagnostic value of this local appearance is due to Dr. Wall.—*Indian Med. Gaz.*, Jan. 21, 1882.

**GENITAL IRRITATION.**—From a study of nineteen cases, Dr. LANDON CARTER GRAY believes:

1. That there is no proof that genital irritation can produce a reflex paralysis.

2. That while it is probable that slight nervous disorders, as incontinence, retention, difficult micturition, erratic movements, and slight nervous disturbances can be produced by genital irritation, the proof is not yet complete.

3. That operations for the removal of genital irritation may be beneficial even in organic nervous disease.

4. That we should, therefore, remove such genital irritation, if it exist in any case whatsoever, and thus give our patients the benefit of the doubt.

5. That in all cases of nervous disorders, with accompanying genital irritation, we should not regard the latter as the cause of the former until all other probable or even possible causes have been rigidly excluded.

6. That operations upon the genitals, even when there be no genital irritation, may prove to be a useful therapeutic measure in certain cases.—*Ann. of Anat. and Surg.*, Feb., 1882.

**IODOFORM IN SURGERY.**—In the *Berliner Klin. Woch.*, December 12, PRIVAT-DOCENT MIKULICZ terminates a paper upon the most recent employment of iodoform in Billroth's clinic with the statement that all the trials made with this substance justify the following conclusions: 1. Iodoform is, for all conditions in which the direct application of an antiseptic is indicated, an excellent means, deserving preference to all other substances hitherto used for this purpose. 2. The iodoform dressing may be used as a substitute for the carbolyzed gauze dressing of Lister, and is preferable to this on account of its simplicity and certainty. 3. The iodoform treatment admits of the antiseptic treatment of a wound, even under conditions that hitherto did not allow of a powerful antiseptis being pursued. 4. In wounds and ulcers already septicly infected, iodoform, as a rule, operates more quickly and certainly than other antiseptics, while it does not irritate the tissues. 5. Iodoform acts in a specific manner on syphilitic, tubercular, scrofulous, and lupous infiltrations.—*Med. Times and Gaz.*, Jan. 7, 1882.

**LITHOTRIPSY AND LITHOLAPAXY.**—Prof. BILLROTH, in an address made before the Wiener k. k. Gesellschaft der Aerzte, stated that before the adoption of Bigelow's method he had performed lithotripsy in 43 cases; in 2 of these the operation had to be abandoned and lithotomy performed, while of the remaining 41, 9 died. The number of operations, the fatal cases included, averaged  $3\frac{1}{2}$  for each case, the length of the sittings being from 10 to 15 minutes. In 6 cases on which he had operated after Bigelow's method, the duration of the operation was prolonged to nearly two hours without causing any unfavorable result. The only patient which he had lost after litholapaxy had died from hæmo-globinuria produced by poisoning with chlorate of potassium.

Prof. DITTEL also stated that he had made use of this method in ten cases, in all with favorable results, and that he believed in future lithotomy would be restricted to children and to cases complicated by an encysted calculus.—*Centralb. f. d. Med. Wissen*, Dec. 24, 1881.

**PILOCARPINE IN DIPHTHERIA.**—M. ARCHAMBAULT has recently made a communication to the Société de Thérapeutique, and gives the results of this method of treatment in twenty cases occurring in children. He found that it was impossible to give pilocarpine hypodermically from the violent terror and prostration nearly always produced. He therefore administered the drug in the form of an injection. He finds that pilocarpine seems to facilitate somewhat the loosening of the false membranes, but that this is the best that can be said about it, and out of the twenty cases he lost twelve. M. Archambault states that in future he will abandon completely the use of this drug.—*Revue Scientifique*, Jan. 21, 1882.

**PATHOLOGICAL ANATOMY OF THE SPINAL CORD IN PHOSPHORUS POISONING.**—M. S. DANIELO, from researches on this subject, gives the following résumé of his conclusions:

1. The alterations of the spinal cord in phosphorus poisoning are of the nature of a myelitis, whether central (peri-ependymal) or diffuse.

2. In acute cases, deposits of blood pigment, noticed for the first time by the author, occur in the central nervous system.

3. Large doses of phosphorus cause a central mye-

litis through the whole extent of the cord, with the formation of blood extravasation and deposits of pigment. Smaller doses, frequently repeated, produce a diffuse myelitis involving both the gray and white matter.

4. By means of phosphorus, therefore, an inflammatory irritation either of the nature of a diffuse myelitis, occupying both the gray and white matter, or an inflammation confined to the gray matter, may be produced at will.

5. The nervous phenomena observed during life, may be attributed to one or the other of these forms of spinal inflammation.—*Bull. Gén. de Thérapeutique*, Jan. 15, 1882.

**TREATMENT OF FRACTURE OF THE CLAVICLE WITH SILVER SUTURES.**—The unsatisfactory results which ordinarily follow treatment of fractures of the clavicle, in spite of the numerous forms of apparatus at the service of the surgeon, have led Dr. LANGENBUCH to attempt the above method. He reports the case of a boy, ten years of age, with a fracture between the middle and outer third of the left clavicle, which he treated by cutting down on the seat of fracture and suturing the ends of the broken bone accurately together with silver wire, and the periosteal sac with catgut sutures; the wires being so twisted that the ends could not perforate the skin, the wound was treated antiseptically, the arm being fixed by a Desault bandage. An ideal result is said to have been obtained.—*Deutsche Med. Woch.*, Jan. 28, 1882.

**MANIA ACCOMPANYING SCARLATINA.**—Mr. R. B. MITCHELL reports in the *Edinburgh Med. Journal*, Feb., 1882, a case of violent mania, characterized by delusions, occurring in a woman, aged fifty years, during an attack of scarlatina. A similar case was mentioned in our issue of Jan. 7, p. 15.

**WRITERS' CRAMP.**—The treatment of functional spasmodic affections in general is so very unsatisfactory, that we should gladly welcome any substantial addition to our therapeutic knowledge from any quarter. M. WOLFF, a German teacher of writing, has earned for himself a considerable reputation by his skill in the treatment of this class of affections; and his success is attested by such authorities as Nussbaum, Bamberger, Benedikt, Billroth, and Esmarch. Hearing of the beneficial results of his system, M. Charcot invited him to Paris, and placed under his care two patients suffering from writers' cramp, who had been treated by himself and M. Vigouroux for some months without any improvement. M. Vigouroux has published the cases (*Le Progrès Médical*, 1882, No. 3), and mentions that, after being handed over to M. Wolff, the first was cured in fifteen, and the second in thirteen days. M. Wolff's system consists in a combination of gymnastics and massage. He makes his patients execute movements in all directions with the affected hand for half an hour to an hour and a half at a time, three or four times a day; and, in addition, the muscles involved are stretched more or less forcibly three or four hundred times daily. He also uses massage and friction, and attaches considerable importance to percussing the affected muscles. The most essential part is the extension of the spasmodic muscles. He thinks, if no improvement is apparent after five or six sittings, the case should be abandoned. The method is worth imitating, though, as M. Vigouroux remarks, while M. Wolff generously explains his procedure, he cannot give us, at the same time, his experience, his practical skill, or that medical instinct which not only enabled him to devise his method, but guides him in its application.—*British Med. Journ.*, Jan. 28, 1882.

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SATURDAY, FEBRUARY 25, 1882.

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## NATIONAL PUBLIC HEALTH LEGISLATION.

### II. THE NATIONAL BOARD OF HEALTH.

FIVE bills, affecting the National Board of Health, have thus far been introduced during the present session of Congress.

One of these, which has passed the Senate and is now before the House, directs the National Board of Health to obtain pure vaccine virus, and to furnish it to all persons applying for it at cost price, and appropriates fifteen thousand dollars for that purpose. Upon this, it is only necessary to remark, that vaccine virus should be not only pure, but fresh; that it should, therefore, be sent direct from the farm to the consumer, and that the National Board would do much more good by having made frequent careful inspections of the establishments of persons furnishing vaccine virus for sale, and by publishing the results of these inspections, than it would by undertaking to buy and sell the article.

The other four bills relate more especially to the quarantine duties of the Board, and are substantially identical. We select for comment the latest one, namely, that introduced by Mr. Van Aernam, the Chairman of the House Committee on Public Health, numbered H. R. 4043, February 6, 1882, and entitled "A Bill to Prevent the Introduction of Contagious and Infectious Diseases into the United States."

The greater part of this bill is merely a repetition of the provisions of the original quarantine act passed in the spring of 1879; the author apparently thinking it better to present the whole original act, as it would appear if amended, than to present merely the provisions for the amendments desired.

It is not easy to understand the changes which this bill would make in the powers and duties of the National Board of Health as regards quarantine, but from a careful comparison with the original act, with the annual reports and recommendations of the Board as regards this subject, and papers read by individual members of the Board before various associations, the following seems to be a fair statement of the case:

On the 1st of January, 1880, after the experience of one season's working of the quarantine act, the National Board of Health presented a report to Congress, concurring in certain recommendations made by a special committee of the National Academy of Science, of which Dr. S. Weir Mitchell, of this city, was chairman.

This Committee recommended "that the act to prevent the introduction of contagious and infectious diseases into the United States, approved June 2, 1879, should be so amended that, in order to enforce the penalties provided in that act for vessels which shall enter, or attempt to enter, ports of the United States in violation thereof, it shall not be necessary to show that the port of departure was at the precise time of departure of such vessel actually infected with contagious or infectious disease, nor that ten days of official promulgation in the port from which said vessel sailed, shall have been effected. It seems preferable that all vessels from foreign ports coming to ports in the United States, shall be required to have bills of health. It is also recommended that the medical officer, who has to serve in the office of the consul at a foreign port, may be either detailed by the President, or appointed by the Board at its discretion."

It does not appear that any action has ever been taken by Congress upon this conjoint recommendation of the Academy and the Board. The bill now under consideration attempts to avoid one of the difficulties met with under the working of the original act, by providing that a formal notification to foreign ports through the Department of State of the provisions of the act, and the rules and regulations made in pursuance thereof, has been made for at least ten days in the port is all that is needed to bring a vessel sailing from that port under the provisions of the law, provided contagious and infectious disease exists there.

This "notification through the State Department," is to take the place of the "official promulgation" in foreign ports required by the existing law, and which has made the law practically a dead letter for certain ports, and more especially those of Cuba, to guard against the dangers from which the law was mainly intended. Official promulgation is, it seems, something which can only be effected with the consent or by the authority of the government



of the port interested, and the Cuban government has thus far refused to give its consent; such consent is, however, not necessary to the notification provided in the present bill, so that this amendment is in accord with the recommendations above referred to. It is probable, however, that this amendment is in itself of comparatively small importance, so long as the proviso that bills of health are to be required only from ports in which contagious and infectious disease exist, remains in the law.

The proper course would seem to be that recommended by the committee of the National Academy, viz., that all vessels from foreign ports coming into ports of the United States, shall be required to have a bill of health, to be signed by the United States consular officers, which is the system of Portugal. The importance of this does not, however, depend so much upon its utility in preventing the importation of contagious diseases, as it does in the tendency which it would have to secure clean ships and the health of seamen; and it is from this point of view largely that we think the general government should legislate in this matter. The National Board of Health does not appear to have again called the attention of Congress to the difficulties in enforcing the act dependent upon the causes to which we have referred, and the reason for this, as given by Dr. Billings, in a paper read before the London Medical Congress, was that it was considered best "to obtain by conference between the nations most interested on international system of prompt and reliable information as to the sanitary condition of ports and ships, which information should in part be carried by the ship itself, in the form of a bill of health, and in part, in the case of a presumably affected ship, preceded in the form of a telegram."

In accordance with this view, the International Sanitary Conference met in Washington a year ago. The full proceedings of this conference have not yet been published, and with regard to the value of the results obtained from it, there seems to be some difference of opinion. It approved the new bill of health proposed by the National Board of Health, which is a simple description of the sanitary condition of the port and of the vessel without any reference to free pratique or quarantine, but we have not heard that, as yet, the actions of the conference have been given practical effect by any of the governments represented.

Mr. Van Aernam's bill also provides for the appointment, by the Board, of medical officers at certain foreign ports, as recommended by the committee of the Academy; and for the sending free through the mails of reports, etc., destined for the Board. It will be seen that very little change is proposed by the bill in question. It must be remembered, however, that the present so-called quarantine act is

to be in force only four years from the date of its passage, in June, 1879. We presume, that if Mr. Van Aernam's bill becomes a law, it will practically do away with this limitation of time, although this is not certain in the absence of a special repealing clause.

#### BRIGHT'S DISEASE IN LIFE INSURANCE.

THE frequency of totally unsuspected Bright's disease is not slight, and for life insurance companies it is a very serious matter. A single undiscovered case may cost them five, ten, or even twenty-five thousand dollars. Every careful doctor knows that he meets constantly cases in which examination of the urine reveals a previously unknown, but evidently long-existing, Bright's disease, which, when discovered, is irremediable, and goes steadily, though it may be slowly, down to the grave. Most of our insurance companies insist on an examination of the urine in all applications for \$10,000 or over; others, more wisely, set the minimum at \$5000; but experience has shown that no risk whatever should be accepted without such an examination. Nay, more, we go so far as to say that prudence dictates that every person of forty years old and upwards should have the urine examined once a year, especially if he have the serious nervous strain which attends heavy business responsibilities or laborious professional work. We go to our dentist annually to see if our teeth need repairing, *i. e.*, to see if any unsuspected disease be arising. At the worst, if we neglected to do so, we would but lose our teeth after years, and have to replace them by a less convenient and more costly artificial set. But if the kidney become diseased —?

It may be said that it would add largely to the cost of medical examinations. True; but, *per contra*, it would save twice or thrice, possibly tenfold, its cost, in the escape from risks that are sure to die within a very few years and entail large losses. One \$5000 loss so avoided would pay the additional fee for 1200 or 1500 such examinations, and the latent disease is vastly more common than that.

#### THE FALLACIES OF HOMŒOPATHY.

WE are glad to see under the above title an excellent article for lay readers in the March number of the *North American Review*. It seems more than likely that the relations between the regular profession and the homœopaths are again to be made the subject of a discussion which will probably excite popular interest and criticism. It is, therefore, well that through some channel the public should be informed of the essential teachings of the homœopathic school and of its proper position viewed from a scientific standpoint.

The difficulty which invariably presents itself at

the outset in arguments directed against homœopathy, is accurately to define and limit the so-called "system," which in both principle and practice is variously represented by its advocates, who in turns, twists, evasions, and sudden changes of base, are only equalled by the famous *pulex irritans* of the immortal Irishman.

It may, however, be taken for granted that a belief in one or all of the following propositions is essential to constitute a true disciple of Hahnemann, and that if they are all palpably untrue, and their falsity is capable of scientific demonstration, his followers must lie under the suspicion of being deficient in either mental or moral calibre:

1. That diseases are dynamic changes in a vital principle, or, in other words, are spiritual entities not depending upon material substances.

2. That the cure of disease is most easily and completely effected by drugs the therapeutic action of which closely simulates the symptoms of the disease itself; and that the probability of a favorable result increases with the exactness of this resemblance or similarity.

3. That the power of drugs increases with their attenuation, which may be carried to an almost unlimited extent.

4. That trituration or agitation confers new and augmented power upon the drug so treated.

To use their own expressions, we may say then that the doctrines of the "spiritual principle" of disease, of "*similia similibus curantur*," of "infinitiesimals," and of "potentization," either singly or collectively, constitute the foundation upon which the teachings and practice of homœopathy rest, and without which there would be no such school of medicine, and no such class of practitioners.

If this be true, it becomes important that we should be able to state clearly and intelligibly the grounds for our disbelief in these hypotheses, and for our rejection of them, and repudiation of their exponents.

The daily press and the community at large are, as a rule, misinformed as to the situation, and are much inclined to applaud the mistaken efforts at compromise on the part of some of the regular profession, or to set down to bigotry and professional jealousy the admirable and unwavering resistance which, for nearly a century, that profession, as a body, has shown to this as to all other forms of quackery.

The absurdity of these dogmas is now generally recognized, and we need not at present repeat their refutation; but even in our own ranks there are doubtless some busy practitioners who, while convinced of the fallacy of homœopathic doctrines, and of the feebleness or insincerity of their upholders, and mentally satisfied with the knowledge of their uni-

versal rejection by impartial scientific investigators throughout the world, have neglected so to inform themselves as to be able to state explicitly and succinctly their reasons for holding these opinions.

On this account we have called attention to the above-mentioned article as affording a clear and fairly complete review of the subject, so written as to be within the comprehension of every intelligent reader.

#### A NEW DANGER IN ANÆSTHESIA.

DR. GEORGE FISCHER, in the *Deutsche Zeitschrift für Chirurgie*, 1881, vol. xv., p. 188, relates a case illustrating a curious and novel danger in anæsthesia which, so far as we know, has not yet been recorded. A patient with a compound fracture of the thigh suddenly died during anæsthesia from chloroform. The chloroform was pure, no food or mucus was found in the mouth, the tongue had been drawn well forward, and on the first appearance of serious symptoms, when but little of the anæsthetic had been given, artificial respiration and electricity had been used, but in spite of all and from no known cause the patient died.

The autopsy explained it all. A piece of chewing tobacco was found in the larynx completely obstructing the glottis. Hereafter not only must the operator remove artificial teeth before giving an anæsthetic, but he must see that a tobacco-chewer has none of the beloved weed hidden in any remote corner of his mouth. At the University Hospital, in Philadelphia, we learn this precaution is always taken, since in one case lately a patient nearly died from the same cause.

#### TRACHELORRAPHY.

"Of the beneficial results of the operation of trachelorraphy, I must candidly admit that I am not now so sanguine as at first." So says Dr. Goodell, and we think the judgment of most gynecologists, if founded on any wide experience, will echo the sentiment. Our opinions, as well as our coats, are largely swayed by fashion. When Emmet first emphasized the importance of lacerations of the cervix, trachelorraphy was to be the panacea for the ills of women, if we had judged from reports of the marvellous results obtained by over-sanguine doctors in all parts of the country. "But," adds Dr. Goodell, "I now know better when to operate, and this fact I have learned: That nervous exhaustion and spinal irritation will evoke symptoms which others, as well as myself, have referred to cervical tears, but which were in no wise dependent on these lesions."

The operation has its sphere, and is a permanent gain to our surgical resources. But it will not do everything. The ebb-tide has set in. In time we shall know just how much it will do, and how much it will not.

**THE NEW YORK CODE OF ETHICS.**

WHILE the New York State Medical Society by the adoption of its new Code of Medical Ethics has deliberately severed its relations with the American Medical Association and the entire profession it represents, its action does not seem to be received with enthusiasm by the New York State Homœopathic Society, to whose members it has extended the hand of affiliation. Dr. Talcott, in his Presidential Address before the State Homœopathic Society, at its annual meeting in Albany, referred to "the remarks made by Dr. Jacobi, President of the Allopathic Society, at its meeting last week, assuming that the homœopathic school was deserting its principles for the sake of affiliating with their brethren of the dominant school, which he said was dominant no longer, as shown by their offer to consult with their formerly despised brethren. What has wrought this? Has Mohammed gone to the mountain, or the mountain come to Mohammed?"

In view of the fact that the new Code of Ethics of the New York State Medical Society is in direct antagonism to the views which the Society has held, without expressed dissatisfaction, for over a quarter of a century, and to the long-standing code accepted by every regular medical society in the United States, the question may well be asked, Was not its adoption on the same day that it was presented and for the first time made public, and without the members having had the opportunity to learn the views concerning it, of the constituency they represented, both precipitate and unwise?

It remains to be seen how far the sentiment of the four thousand five hundred regular physicians of the State is correctly represented by the hasty action of the fifty-two gentlemen whose votes have given them this new code, and isolated them from the profession of the country.

In England the subject of consultations with homœopaths has attracted of late considerable attention in connection with the case of the late Lord Beaconsfield, and the views of the profession were recently formulated in the resolution passed by the Royal College of Physicians, which has already been published (see number for January 21, p. 86). In another column we reprint from the pages of the *Lancet* an admirable article on "Quackery within the Profession," which deserves the widest publicity. It is so timely, and deals so properly with the subject, that we are sure its perusal will afford gratification to every reader of *THE NEWS*, and it well shows, as is formally maintained by the Royal College of Physicians of London, how impossible it is for an *honest* physician to consult with any one who trades upon designations implying a special system of treatment.

**THE TRANSACTIONS OF THE INTERNATIONAL MEDICAL CONGRESS, LONDON, 1881.**

WE have just received by post the four large octavos containing the transactions of the late International Medical Congress in London, prepared by the Hon. Secretary-General, Sir William MacCormac, assisted by Mr. Makins and the secretaries of the sections. We are glad to write *Sir William*, for any one who was present at the Congress is well aware that the wonderful smoothness of its machinery and its successful results are due above all to the untiring energy and marvellous tact of the Secretary-General. Were any other evidence needed of the fitness of the honor conferred upon him it would be found in the wonderful celerity with which these volumes have been issued. In less than five months after the Congress adjourned its entire transactions are issued. What a labor this involved will be seen in the following facts. There are nearly 2600 royal octavo pages, containing 450 papers published in English, French, and German, with 858 speeches from the discussions, all printed in English. These had to be written out, often after translation, submitted to the speakers for correction, many of the papers had to be judiciously condensed (and some were even omitted, a liberty we commend to other Hon. Secretaries-General), proof had to be read and re-read, 180 illustrations had to be prepared, and after all this was done came the printing, binding, sending a circular to all the members and medical journals giving information of the publication of the volumes, modes of payment, etc., addressing each volume, and the transmission by mail. All this has been done, and successfully done, in less than five months.

Of the contents of the volumes we cannot speak at present, except to say that they are worthy of the occasion and the authors. Could we say more?

**LEGAL PROTECTION OF THE PUBLIC AGAINST CONTAGIOUS DISEASES.**

WE are very glad to see that the Philadelphia Board of Health have taken action in the matter to which we alluded in our issue of the 4th inst. They have recommended to Councils ordinances to prevent the spread of contagious diseases and providing for fines, as follows:

"Twenty-five dollars for the use of a public conveyance, without notification to the owner, by any person suffering from a contagious disease; \$50 for a public funeral; \$25 against any victim of a contagious malady for entering any public street car or conveyance; the same for selling any bedding, carpets, or household effects from an infected residence; and a fine of \$10 for failing to state the cause in authorizing the publication of a death notice under similar circumstances; letting any infected house, or



room in such house, renders the offender liable to a fine of \$100." Clearly as to the first item the fine alone is not enough. The carriage should also be disinfected by the medical officer of the Board of Health free of charge, and a fine of \$25 should be imposed on the owner, if it be used again before such disinfection.

We hope that all of our city readers will immediately use their influence with the members of Councils from their various wards, in order to have these reasonable and eminently necessary provisions made for the protection of the community.

## REVIEWS.

CHEMICAL ANALYSIS OF THE URINE, BASED IN PART ON (CASSELMANN'S ANALYSE DES HARNES). By EDGAR F. SMITH, Ph.D., Asa Packer Professor of Chemistry in Muhlenberg College; JOHN MARSHALL, M.D., Demonstrator of Chemistry, Medical Department, University of Pennsylvania. With illustrations. 8vo, pp. 104. Philadelphia: Presley Blakiston, 1881.

It is not always pleasant for THE NEWS to tell the plain truth about books that come to it for review. But we think it would be far better for both the makers and buyers of books, if candor always ruled its action. It must not be supposed, therefore, that we mean to be unkind in the present case, because we feel called upon to tell the plain truth about the little book before us. Our first comment is that its style is atrocious; for example (p. 50), "... Albuminuria such as is observed in Bright's disease, in nephritis, neoplasma renis. The so-called Bellinac casts, pus sediments in acid reaction and a small quantity of neoplasms even, always distinguish each of these troubles introducing albumen."

Again, it is very apparent that the authors have had but little experience in the practice of medicine. What they say about clinical deductions, is gathered from German authors, and very badly gathered at that. Again, their description of tests is confusing, inexact, incomplete, and sometimes erroneous. Had they taken the pains to ascertain, they would have found that the indigo-carmin test for sugar is worthless, as the characteristic reaction may be obtained when there is no sugar present.

Again, a proper acquaintance with uric-acid crystals, or with the German language, would have prevented their falling into the error of translating from Hoffmann and Ultzmann's book (without any acknowledgments) *Wetzsteinform* as "Wetzstein's form," instead of *whetstone-shape*. A similar blunder in observation and in linguistics occurs where another clumsy translation states that calcium phosphate crystals are "keel-shaped," whereas, the crystals are, and *keilförmig* means, wedge-shaped!

We have not had time to hunt up all the sources of the authors' information, but if the rest are as badly translated as the writings of Ultzmann, the poor quality of this book is easy to account for.

In the matter of illustrations, we find that, with one or two exceptions, the plates are inferior copies of those in the beautiful atlas of Ultzmann and Hoffmann, for which they themselves made the drawings, but which are here credited to Casselmann.

On the whole, it looks to us as though Casselmann had made too free use of Ultzmann and Hoffmann's

work, and Marshall and Smith had made too free use of Casselmann's.

In conclusion, we regret to be compelled to say that this applicant for scientific favor lacks the most important qualifications of a guide to either the student or the practitioner.

1. TWO CASES OF VARICOCELE, WITH UNDEVELOPED TESTICLE, WITH REMARKS ON THE NATURE OF VARICOCELE; AND
2. A CASE OF ANTISEPTIC OSTEOATOMY OF THE TIBIA, IN WHICH RAPIDLY FATAL CARBOLIC INTOXICATION OCCURRED. By A. PEARCE GOULD, M.S. Reprint from the London Clinical Society's Transactions for 1881. 8vo, pp. 23.

Whether a varicocele causes atrophy of the testicle, or whether it merely prevents the development of the testicle, is a mooted point, with the preponderance of authority in favor of the causal relation of the varicocele. Yet those who believe that the testicle is not atrophied, have Sir Astley Cooper and Sir James Paget to appeal to. The difficulty is in the establishment of the real facts, *i. e.*, whether the testicle was originally small and undeveloped before the varicocele began; or, whether, having been of good size and firmness, it wasted after the appearance of the varicocele, and became flabby, etc. The statements of patients are, as a rule, so little to be trusted, at least on a point requiring exact observation, that the facts elude us in nearly every case. Mr. Gould's cases all seem decidedly to favor the view that the varicocele has prevented the development of the testicle. As to the cause of varicocele, he believes that, in many cases at least, it is not due to internal pressure and want of external support, but is a real venous hypertrophy. He suggests as a reason for it, that at puberty, when the testicle rapidly develops, there may be a transference of the developmental stimulus from the testicle to the veins. Mr. Gould does not seem to be aware of the paper of Dr. John H. Brinton, of Philadelphia (*Amer. Journ. Med. Sci.*, July, 1856), in which he asserts that there is no valve at the junction of the left spermatic and renal veins, and that this is one of the factors in producing varicocele on the left side, as the right vein at its junction with the vena cava has one. Mr. Gould states that in "all cases" he has yet examined on the left side, a valve has been found, either single or double. We are not disposed to support our American confrère against facts, but certainly our experience coincides with his.

The case of a simple osteotomy in which apparently death followed from cutaneous absorption of the carbolic acid, is certainly both curious as showing the intolerance of some people to the acid, and instructive as to the proper treatment in all similar cases—the immediate removal of the dressing.

ZUR KENNTNIS DER MIKROKOKKEN BEI AKUTER INFECTIOSER OSTEO-MYELITIS; MIKROKOKKENHERDE IM GELENKKNORPEL. Von MAX SCHÜLLER, in Greifswald.

MICROCOCCHI IN ACUTE INFECTIOUS OSTEO-MYELITIS; MICROCOCCI IN THE ARTICULAR CARTILAGE. Reprint from the *Centralbl. für Chir.*, 1881, No. 42.

The presence of the lower forms of life in many diseases is now a question that is being investigated with far more than the former vigor. Lücke, Klebs, and others have found micrococci in the marrow of the periosteum and the surrounding soft parts; a fact that Schüller not only confirms, but adds to it the discovery of their existence in the articular cartilages, where they cause the destruction of the tissue, and give

rise to small canals leading down to the diseased bones.

The subject is well worth further study for its possible explanation of the septic results so often following osteomyelitis.

## SOCIETY PROCEEDINGS.

### OBSTETRICAL SOCIETY OF PHILADELPHIA.

*Stated Meeting, February 2, 1882.*

E. L. DUEK, M.D., PRESIDENT, IN THE CHAIR.

*(Concluded from page 198.)*

*Death after Dilatation of the Cervix Uteri with a Laminaria-Tent.*—Dr. B. F. BAER reported the following case: In September, 1880, I was asked by Dr. Ch. K. Mills to see Mrs. M. K. She was *æt.* thirty-two, and had had six children, the youngest having been born four years previously. The last labor had been tedious from uterine inertia, and was followed by a smart postpartum hæmorrhage. Since then she had suffered from a recurrence of metrorrhagia about every two weeks, lasting from a week to ten days, each time, and very profusely. The blood-loss was attended by severe uterine tenesmus. In the intervals between the hæmorrhages there was a constant mucous leucorrhœa. She complained of pain in the hypogastrium and back. Her appearance indicated anæmia and loss of flesh.

Physical examination elicited the following condition. The perineum was slightly lacerated and the vagina relaxed. The cervix uteri was large, soft from congestion, and lacerated bilaterally, with a swollen, everted, and abraded mucous membrane. The body of the uterus was hypertrophied and retroverted, with a slight flexion at the junction of the body and neck. At the point of flexion there was such rigidity of the posterior wall, probably from atrophy, that, when the uterus was replaced, it would return to its old position as soon as released. The sound passed three and a half inches, and proved the uterine cavity to be dilated, and its walls soft and rugous. The internal os, the point of flexion, was quite narrow.

This lady had received proper general medical treatment from her brother, who is a prominent physician, and from others, and during the year previous to coming under my care, she had received local treatment, but with no benefit, so far as the metrorrhagia was concerned.

I diagnosed, as the cause of the hæmorrhages, a hypertrophied, granular condition of the endometrium, the result of subinvolution and retroversion of the uterus, with possibly a polypus.

The plan of treatment pursued was absolute rest, vaginal injections of hot water, reposition of the uterus by placing the patient in the knee-chest position and scarification of the cervix, followed by an application of Churchill's tincture of iodine once every six or seven days. Ergot and the tincture of nux vomica, with quinia and some other remedies, were administered internally. The constriction at the internal os made it difficult to medicate the cavity of the uterus properly, and it was thought more prudent to await the result of less radical measures. The result, however, was not gratifying, for the hæmorrhages continued to recur with as much, if not more, severity than before. I now introduced a laminaria-tent and allowed it to remain about twenty hours. I then removed it with some difficulty, for it was grasped firmly by the internal os, the contraction at which point it had failed to fully overcome. The canal was now patulous enough to permit me to pass the dull wire curette, with which I removed, very easily, a large quantity of hyperplastic or granular mucous membrane. But my patient's temperature was

up to 100° before I removed the tent, and her general appearance indicated trouble ahead. It is sufficient for my purpose to state that, in spite of every effort to combat it, the temperature continued to rise and the case gave every evidence of septicæmia with metritis and perimetritis, and went from bad to worse, until death took place nearly three weeks after the introduction of the tent. Full precautions were taken to guard against septicæmia.

Why did my efforts to benefit this lady end so disastrously? Ought I to have waited, and given a further trial to less dangerous means of treatment? The patient had been in the care of a competent gentleman for a year before, and he had pursued that plan most faithfully, with no relief, and my own treatment of the case, before I introduced the tent, had continued over a period of two months, and with a like result; the patient was going downhill, and something more had to be done.

Was the result due to the use of the curette? I do not think so, for the material removed was soft and degenerated. It would have been bad practice to have allowed it to remain.

Should I have used a steel dilator instead of a tent? The tent was used for the purpose especially of softening the indurated tissue at the internal os. The steel dilator would not have done that; and it was very desirable that the os should be made patulous by softening of the tissues, so that the body would contract and disgorge the vessels in its walls. The steel dilator would not have fulfilled the indication so well, but it would have been a less dangerous means of dilatation.

Was the operation deferred too long, until, from the loss of blood, the vessels were eager to absorb any fluid which came within their reach, and the blood itself so disabled that it could not purify itself when poisoned? I believe that comes near the truth.

I do not report this case as an argument against the use of tents, but because I believe all such cases should be reported, so that we may not be led to regard dilatation of the cervix as a simple measure devoid of danger.

This is the only case of death I have had to follow dilatation with the tent. I have never had a case of death follow dilatation with the steel instrument.

Dr. E. L. DUEK described a method of preparing a sponge-tent expeditiously. Take a clean sponge of cylindrical form, dip it into melted wax or paraffine, and compress it into form as it cools. Tents may be introduced when the speculum and forceps are not at hand, by wrapping the string attached to the tent around the forefinger of the right hand, and inserting the thumb nail into the base of the tent. The first and second finger of the left hand are passed behind the cervix; the tent is then introduced into the os uteri, and the left hand being quickly transferred to the abdomen, counter-pressure is made and the tent forced home. Pain following the insertion of a tent is frequently the consequence of pressure upon the fundus, and if the tent be withdrawn about one-fourth of an inch, the pain will be relieved. The sponge-tent is, without doubt, one of the most powerful means for the reduction of uterine hypertrophy.

Dr. PAUL F. MUNDÉ agreed with Drs. Smith and Duer, that sponge-tents were indicated in uterine hypertrophy and granulations of the endometrium. He has never had any bad results from dilatation of the uterus by mechanical dilators of any form; but he has withdrawn very offensive sponge-tents after twenty-four hours' use, and feared danger might be near, and wished to avoid it. He can get tupelo-tents of any size; they dilate not too rapidly, but regularly and strongly, and he preferred to use them, as he was afraid of sponge-tents. He agreed with Dr. Smith as to the method of application. He always made use of three steps in the insertion of a tent. He placed the patient in Sim's

position. The cervix should be exposed properly, and seized by a tenaculum; then the tent being properly held in a strong forceps, is dipped first into a jar of liquefied carbolic acid, then into a jar of vaseline, and then rapidly passed into the previously cleansed uterine cavity; quickly, if the tent be not too large, for if there be any point for it to catch upon it will catch. At the end of twenty-four hours he always removed the tent, and they were very nasty sometimes. He always dreads some bad result, but has been fortunate so far, and has not seen any. He now uses the tupelo-tent, and treats it in the same manner that he previously did the sponge-tents. It is easily introduced, it becomes fastened in a few minutes, its effects are good, the patient does not complain of much pain, it does not imbibe so much as sponge, and does not sink into the uterine tissue as the latter does, but it is not so efficient in reducing the size of the hyperplastic uterus, for there is nothing else so good as sponge for that. After a tent is removed, the uterus should be thoroughly cleansed. In cases where the sponge was successful for the relief of sterility, a tupelo- or laminaria-tent would have probably done just as well. A sponge-tent increases discharge and causes local irritation, and its removal involves loss of epithelium, and for these reasons it is not generally so good for relief of sterility. As the sponge-tent is rough, it sticks, and is introduced with great difficulty, if it be of large size, in proportion to the calibre of the internal os. It is a maxim that a sponge-tent must not be introduced into a fresh wound, and does not the dilator or bougie cause a fresh wound? The sponge-tent is undoubtedly the best for hyperplasia, but all the other indications are filled by the laminaria- or tupelo-tents. He had experienced the same difficulty as Dr. Smith in the tapering-tent, and had, therefore, cut off the small end of the tent. A Molesworth dilator is open to the same objection in some cases, not dilating either os, but expanding largely in the space between; the conical-tent does the same thing; blunt sponges are very difficult to insert; the laminaria has dilated in the cavities of the cervix and body of the uterus with an hour-glass constriction at the internal os, and it was withdrawn with great difficulty. The tupelo-tent dilates more equally, and also more slowly. Sponge-tents are also hard to withdraw, and should be twisted before traction is made.

Dr. DUER suggested twisting in one direction only, allowing the grasping instrument to be drawn in by the shortening during twisting, until the tent was entirely loose. On one occasion, in his experience, a piece of tent broke off and remained inside the uterus, but it was extruded by uterine action, and was found in the vagina the next day.

Dr. J. CHESTON MORRIS had found out the uselessness of conical-tents, unless inserted in a reversed position; he prefers the cylindrical form. The cases of death from sponge-tents were probably due to the use of three successive tents, at intervals of twenty-four hours; he prefers to allow a tent to remain from forty-eight to seventy-two hours; he has never had any serious results, but in consequence of the tents being disagreeable and troublesome, he now uses Molesworth's dilator, and with great satisfaction; but they are badly made, they leak, and are apt to break under necessary pressure. In one case he burst three dilators before he succeeded in effecting complete dilatation. He agreed in the usefulness of this method of treatment for hyperplastic enlargement, and thinks that in many cases it acts in imitation of a miscarriage—expansion, then contraction with the aid of ergot will cure chronic metritis and enlargement. He is now using large soft-rubber stems to effect a similar purpose; they gradually overcome the resistance of the internal os and expand it. He has never used the tupelo-tent. In his opinion, it is far

safer to allow a tent to remain two or three days than one only. In removing a tent, push in slightly at first, and then make traction with a curving motion. He thinks the position on the back far easier for introducing a tent than Sim's position. He has introduced tents in the manner described by Dr. Duer, without speculum or forceps.

Dr. MUNDÉ asked, Where should tents be applied? At the office? What should be done with the patient after the insertion of a tent? It is very reprehensible to introduce a tent unless a patient can be kept in bed for one or two days after the removal of the tent; this is a very important point. He is accustomed to introduce all tents in the Sim's position, and through Sim's speculum, and has not succeeded so well on the back because the tent easily becomes rough if not quickly pushed to its place. He introduces the sound, and frequently the hard stem on the back, pushing the uterus down over the stem by pressure above the pubes.

Dr. A. H. SMITH said that success in the treatment of sterility, by means of sponge-tents, depends upon the relation of the time of insertion to the menstrual period. If used just before the period, it dilates the uterus, and expends all its malign influence before the time at which the uterus is expected to receive the impregnated ovum. The tupelo-tent fails in fulfilling the indications, as it would not allow the flow to pass through it (unless perforated), and it could not pass beside it if large enough to be of benefit. He had commenced to use tupelo-tents when they were first introduced; he found them very spongy and soft, with slight dilating power, and easily constricted by the internal os; he did not find it to have any advantages over the laminaria- or sponge-tent. It has great powers of absorption, and had the appearance, when new, of having been used and dried again. According to Dr. Mundé, the tupelo-tents are much better made now, are hard, smooth, and have greater power.

Respecting the use of the bougie before inserting a tent. The bougie does not cause a tear or abrasion of the surface. The wax bougies are flexible, pointed, and graduated in form. He has never seen bleeding follow their use; he has never used a steel bougie. He considers that there is far less risk than from the use of mechanical dilators, of which he is much afraid.

There is no difficulty in introducing a cylindrical tent, as it dilates uniformly from end to end, and a smaller tent answers the purpose, as the important point is the internal os; that is where the largest amount of tent is needed. He prefers the position on the back, the relation of parts is more natural, and the uterus is more easily straightened by pressure on the fundus above the pubes, making introduction easier. Tents should never be introduced in the office; to do so is very reckless. The patient should be put to bed, and cleansing injections should be used.

On motion, the further discussion of mechanical dilators of the uterus was postponed to the stated meeting of March 2.

#### NEW YORK ACADEMY OF MEDICINE.

*Stated Meeting, February 16, 1882.*

FORDYCE BARKER, M.D., PRESIDENT, IN THE CHAIR.

After various reports had been read and adopted, the PRESIDENT introduced the author of the scientific paper of the evening, Dr. A. A. SMITH, who proceeded to read his paper, entitled:

*Clinical Observations upon Diabetes Mellitus, with Cases.*

Detailed histories of five cases were presented, a summary of which is as follows:

First, all were cases of disturbances of the nervous system; in none was there injury or sudden



shock to the nervous system, but continuous mental strain; they all occurred in private practice, and among people exceptionally well-to-do. Second, all had disturbances of hepatic and gastric digestion. None had albuminuria, nor malaria, most were addicted to the use of alcohol. Two had a gouty tendency, three had none, and none had rheumatism. There was not great emaciation in four, it was great in one. None ever had any troubles with vision referable to the diabetic condition. None ever had any skin troubles either during the progress of the disease, or as a sequel. Hereditary tendency was traceable in only one, and that very remotely. There was in none any increase in appetite. In only two was there any great excess in the quantity of urine passed. Three gave a history of excessive thirst.

These cases, said Dr. Smith, seem to illustrate the necessity of selecting a plan of treatment and following it up persistently. The treatment adopted in all of the above cases was the same, and as follows: Codeia, one-fourth grain, gradually increased to one grain, three times a day, after meals. Also, the tincture of chloride of iron, twenty drops, three times a day, after meals, not increased. These patients were given a laxative pill consisting of aloes, nux vomica, rhubarb, and extract of hyoscyamus, sufficient to relieve constipation, which was a prominent symptom. They were furnished with a written diet list as follows: Vegetables, tomatoes, celery, cabbage, lettuce, cucumbers, pickles, spinach, radishes, mushrooms, cauliflower, asparagus, truffles, oyster plant, onions, watercress, olives, tea and coffee without sugar. Gluten bread made from the gluten flour of the New York Health Food Company, also almond-flour bread. They were allowed to drink as much water as desired, to take meats, fats, oils, cream, butter, poultry, fish, eggs, cheese, and permitted to drink milk. Of drinkables, brandy, whiskey, claret, burgundy, very dry sherry, the acid wines from the Rhine valley, were allowed. They were privileged to eat nuts of all kinds.

One patient was still under observation; four had entirely recovered. Of the four that had recovered, one had had no sugar in the urine for seven years, and three, none for two years. All had resumed their former occupations, and were as actively engaged in work as ever. Treatment was continued about four months after the disappearance of all evidences of sugar in the urine, and, for the most part, up to the present time the tincture of chloride of iron has been continued on account of the anemia.

One of these patients was a lawyer, another a literary man; one, an artist; one, a railroad man, having charge of a large transportation business, and the fifth, a lady, living in her own home. Their respective ages were forty, forty-six, forty-nine, sixty, and sixty-three years.

The author of the paper was inclined to believe, admitting that diet probably played an important part, that codeia must also be given a very important place in the treatment of the disease. He referred to the fact that opium had probably been used since the second century, but he gave to Pavy the credit of having first suggested the use of codeia. Dr. Smith took the ground that certainly in the majority of cases diabetes has its origin in the nervous system. As an explanation of the good effects of codeia, he suggested its action as a direct sedative to the nervous system, not only in a general way, but to the pneumogastric nerve, accepting the theory that disturbances of the glycogenic function of the liver in diabetes was probably due to influences conveyed from the nervous system through the pneumogastric. He looks upon disturbances of the glycogenic function as entirely secondary. He has no sympathy with those observers that think codeia simply diminishes the quantity of sugar in

the urine, and the quantity of urine, and has no permanent effect. Many cases are recorded apparently cured from the use of codeia, and now Dr. Smith adds four more, and one still under observation, with favorable progress thus far. He states there are probably many cases of diabetes mellitus which go unrecognized, and which, if treatment was begun sufficiently early, could be cured. In many instances patients affected with this disease did not seek the advice of a physician until organs were involved which made recovery impossible. Dr. Smith claimed that the fact that codeia acted beneficially in all these cases in which disturbances of digestion was a prominent feature, adds another argument to those already adduced in favor of a nervous origin of the disease; the explanation of this view being that, ordinarily, codeia produces slight disturbances of the digestive system, but in these cases its administration was immediately followed by improvement in digestion. The question was asked, Is it not probable that in cases of diabetes mellitus, in which emaciation occurs very rapidly, there is interference with the functions of the pancreas, either as cause or effect? Dr. Smith stated that in two of the cases related, he was much surprised upon examination of the urine to find sugar, and that probably the diagnosis in these cases would have remained obscure for some time longer if he had not conformed to his usual custom of examining the urine of almost every patient. The results of treatment in these five cases ought to encourage us to take a more cheerful view of diabetes than has been taken heretofore.

Dr. AUSTIN FLINT, Sr., responding to the request of the President to open the discussion, said, I am glad this disease was made the subject of a paper, for it possesses a good deal of interest as well as practical importance. Diabetes mellitus is either a disease of much greater frequency than formerly, or it is more frequently recognized than hitherto. I certainly have seen within the past five or six years almost more cases than in the whole of my previous life, and the number of histories that I have collected is so considerable, that I have decided to study them analytically, and make them the subject of a future paper. I suppose the disease was formerly very often overlooked, in the first place, for the reason mentioned in the paper, that examination of urine for sugar was not so common a practice as now, especially when there were no circumstances suggesting diabetes mellitus, physicians being contented to look for albumen and casts. Another reason is, that in some stages of the disease there may be no increase in the quantity of urine passed, or excessive thirst, or any other symptom to excite attention to its existence. I have met with several cases where the patient had been ill for a considerable time, and was suffering from some other disease, perhaps a fatal disease, at the time of coming under observation, in which an examination of the urine showed the presence of sugar. I could refer to several cases in this city in which, by a kind of accident, an examination of the urine was made without any fixed idea that this disease would be discovered, and it has been found to exist. I will mention one instance of this kind which occurred some time ago. A clergyman from another State, apparently in full health, came to me with this statement: "I want to state to you a symptom which I have, which may be the most trifling thing in the world, but it has given me some annoyance, so I have come to ask you about it." The symptom was a kind of itching in the penis, and to quote himself, "I have no confessions to make, but did not know but it might have some significance." At first I thought the matter was hardly worth while for consideration, but I finally made an examination of his urine and found it loaded with sugar. The patient recovered in a short time, and two years

have elapsed without a return of the disease. I, therefore, very strongly recommend the adoption of the plan referred to in Dr. Smith's paper, that of making an examination of the urine in all cases. I have seen a number of cases in which the quantity of urine and its specific gravity was not increased, and under these circumstances, not a notable degree of thirst. I am also glad to concur in the statement of Dr. Smith respecting the grounds for a favorable prognosis in a considerable number of cases of diabetes. I have notes of quite a number of cases in which the recoveries have been persistent. I have found that the dietetic treatment will cause the sugar in the urine to disappear or be reduced to a very small amount in a short space of time. In this connection, I will say that the important treatment seems to be dietetic, and I think that failure, in many cases, is due to want of thoroughness in its being carried out; I do not mean that patients do not rigidly adhere to the diet, but that the diet should be made to be satisfactory to the patient. My rule, for a considerable time, has been to give to these patients a list of every article of diet which they may take, and every article which they must not take, so that they can have it before them. If the physician be content with general statements, the treatment will be pretty sure not to be carried out. The great difficulty is in regard to bread; very few patients will eat bread containing no starch. They soon tire of it, and finally it becomes impossible for them to get along with it. I, therefore, give them an article which is not entirely free from starch, though it contains but little. This is prepared by the New York Health Food Company. With this they get along very well. If the matter of bread can be disposed of, the other articles of food are so varied that patients can make a satisfactory menu.

As regards medicinal treatment, I see from Dr. Smith's paper that he advocates the use of codeia, as recommended by Pavy. I have only used it as a form of opiate to relieve weariness of limbs, cramps, and insomnia. I have not depended on it for a cure. I suppose most present are informed of the remedy which has been suggested within the past few years, and advocated by Dr. Husted. I refer to the sulphide of calcium. I confess that I think he was led by his enthusiasm to overestimate the value of the remedy. Still, I have prescribed it in a considerable number of cases; and though I have not sufficient data from which to draw definite conclusions, yet I do feel that the remedy has a certain amount of curative power, and I commenced its use with a good deal of scepticism. I have used it in at least a dozen cases within the last eighteen months.

I will refer to one other point, and that is a toxical agency, in connection with sugar in the urine. I have met with several cases of this kind. The first was that of a gentleman from the South, who came to this city to purchase goods. He had diabetes, and his physician gave him a letter to me. Not feeling well enough to leave the hotel he sent the letter to me by a messenger, with a request that I call upon him. I did so, and found he had diabetes, but his general appearance was healthy. He had taken cold, complained of a sore throat, and had a little pharyngitis, but I did not attach the least gravity to his case. Afterwards he sent for me again, but it was not convenient for me to see him, and I sent my clinical assistant instead. My assistant soon returned, much alarmed, stating that he found the patient in a dying condition. I hastened to the hotel, but he had died before I reached there. I did not observe the symptoms in this case, and the only knowledge I have of them is a few general statements. It was certain that the urine was sufficiently abundant to eliminate the idea of death from ureamic poisoning.

Dr. Flint related the history of one other similar case,

in which death occurred rather suddenly. Some attention had been given to this subject of late. He said there was certainly a liability to the development in this disease of some toxic lesion, which might destroy life in a very short time.

Referring to a favorable prognosis again, he said, I am cognizant of at least half a dozen persons in this city who have diabetes and who are in the enjoyment of comfortable health and doing laborious work, most of them finding it necessary to observe more or less strictly the dietetic treatment. With proper care and proper means, for it is pretty difficult for a poor man to carry out the treatment, I do believe, that by dietetics alone, in a certain number of cases, if not a complete cure, the holding of the disease in abeyance, will be effected, and the general health remain excellent.

Dr. HUBBARD was next called upon by the Chair, and remarked that he had seen several cases of diabetes, and now had two under treatment, one having been under treatment for four years, and the other for two. The dietetic treatment was being used in connection with the administration of yeast. In one case sugar had on several occasions disappeared from the urine. Both patients were ladies in easy circumstances, and had not had, to the Doctor's certain knowledge, any very great mental strain. The one who has been under treatment for four years is now in tolerable health. She had lost some flesh it was true, having been reduced from two hundred pounds to one hundred and seventy pounds. She is about fifty years of age. When treatment was commenced she was passing eight quarts of water. A tablespoonful of brewer's yeast was given three or four times a day. The case of a boy two years of age having diabetes was mentioned. Bicarbonate of soda was administered, but the patient died within a few months. None of the Doctor's cases had been cured, but some had been materially helped.

Dr. VIRGIL B. GIBNEY, being called upon by the Chair for his knowledge of the occurrence of diabetes in connection with organic diseases of the nervous system, said he knew of only one such case, that of a girl having infantile paralysis. In this patient diabetes did not develop until she was about fourteen years of age, at which time she had complete palsy of the right leg. She came of a decidedly neurotic family. She finally died of pulmonary oedema, but no definite connection between the diabetes and paralysis could be made out, and their co-existence was looked upon as an accident.

Dr. KINNICUT remarked that it seemed important to bear in mind that there are, apparently, two distinct forms of the affection. In one the patient can digest only a small quantity of sugar, and these cases can be readily controlled by strict dietetic treatment. In the other there is a serious diversion of the glycogenic function of the liver; and as these cases are never fully controlled, a guarded prognosis should be given. It is in these cases that very slight causes will produce that state known as diabetic coma, or collapse, in which the patient, almost without warning, passes into collapse, and dies.

Dr. PETERS had for many years adopted the view that the disease originated in the nervous system. Pricking the floor of the fourth ventricle would cause sugar to appear in the urine. Cutting the anterior roots would be followed by temporary diabetes. Cutting the posterior roots leads to a more severe and persistent form of the disease. Dr. Peters thought the basis of the disease was a vaso-motor paralysis in the severer forms, and a vaso-motor paresis in the milder forms. He had been in the habit of looking upon nux vomica as the best remedy. He had seen its use followed by benefit in a good many cases, and cure in some. He

concurred with Dr. Kinnicut in the view that in about one-half of the cases dietetic treatment was beneficial or curative; while in the other half, for the most part, only very slight benefit was to be derived from it. A strict anti-sugar diet might be adhered to, and all the symptoms go on. He thought in young children the disease was generally or frequently fatal. In advanced life it is milder, and much more amenable to treatment. In middle-aged subjects usually occurred the most serious cases. There were two large classes of patients. One, stout and obese; this class being quite manageable. The other, thin, nervous, and more or less debilitated, and very unmanageable. There were either two forms of the disease, or two stages, the first stage being manageable, the second stage being unmanageable.

The PRESIDENT alluded to a case, the history of which seemed to have some bearing on the etiology of the disease. The patient, a distinguished baron, distinguished as a poet, writer, and prominent member of the House of Lords, came to this country four years ago, bringing a letter of introduction from a most eminent physician of London, in which he stated the bearer had diabetes. The patient was seen the day after his arrival and was found excessively prostrated, and passing a large amount of urine—three or four quarts in the twenty-four hours. He was a very active man, a free liver, drinking abundantly of wine at meal-time which he supplemented with two or three glasses of water at bed-time. It was extremely difficult to restrict him to any diet, though he carried out the one prescribed to a considerable extent. He visited all parts of the United States, and during the time he was here the sugar in his urine diminished in quantity, and the quantity of urine passed became almost normal. He was permitted to satisfy his thirst by drinking all the water he wished. Since his return to Europe, Dr. Barker has had a long talk with his physician, who states that when his time is occupied with attendance upon great dinners, receptions, etc., he being very prominent socially, he very rarely suffers from diabetic symptoms, but after he has made a great public speech, as in the House of Lords, or has been engaged in historical or literary work, or bringing out a new edition of his poems, there would always be a great increase in the quantity of urine passed, and in the amount of sugar it contained. He would become excessively nervous and irritable. But after his intellectual work was finished and he resumed what we sober Americans might call dissipation, he apparently got well. He now enjoys pretty good health.

Dr. ALFRED C. POST inquired if any of the gentlemen present were informed as to the frequency of gangrenous troubles in connection with diabetes.

Dr. FLINT recalled to mind three cases of gangrene of the lower extremities in diabetic patients.

Dr. PETERS referred to a diabetic patient who received an injury to his great toe and died of gangrene.

Dr. WM. H. DRAPER said: I was unfortunate in coming so late as to be deprived of the pleasure of listening to the whole of Dr. Smith's paper; but from what I did hear, I judge he accepts the nervous theory of the origin of diabetes, and, it seems to me, we must all accept this theory when we consider the experimental researches made upon animals, and, also, when we consider the clinical history of cases of diabetes that come under our observation. There have been a very considerable number of cases of diabetes recorded as the result of traumatism of the brain, and in very many of the cases that come under the observation of the physician in his daily practice we can trace some nervous shock as the beginning of the disease. It would appear from the experimental researches, and from the clinical histories we have recorded, that the range of

nervous irritation which will produce diabetes is extremely wide anatomically, and wide as to the cause of the irritation. But after all the liver appears to be the organ which is the chief instrument in the production of this disease. What remarks I have to make, sir, will be in regard to the association of diabetes with another form of disease, which is more common than diabetes, that disease which Bence Jones described as the "sour disease" in contradistinction to the "sweet disease." In other words, I think the clinical observations of most physicians will show that the great majority of cases of diabetes are associated with lithemia; that there is an error in the digestion of nitrogenous foods, as well as in the digestion of carbonaceous foods, in diabetic subjects. I think it can be shown that many cases of the grave form of diabetes, that form in which dietetic treatment does no good, give a hereditary history of gout, or have illustrated it in their own persons. It will be found, also, in these cases that there is not only sugar in the urine, but very often an excess of urea and uric acid. When we come to the consideration, however, of the milder forms of diabetes, the intermittent forms, I think this association becomes even more marked, because we can almost always in these cases trace in the individual some dyspeptic symptoms associated with gout, and we find in the urine, very frequently, deposits of uric acid and an excess of urea, as well as the presence of sugar. We shall find, also, in these cases, that the patients have been the subjects of true gouty lesions, of cutaneous lesions, of catarrhal lesions, of arthritic lesions. There is another circumstance in the history of these cases, which, it seems to me, goes to show the association of these two maladies, apart from hereditary influences, and apart from the occurrence in both of a similar kind of dyspepsia, in which there is an inability to digest any considerable quantity of carbonaceous foods, and that is, that diabetic and gouty patients are relieved by the same kind of diet, and by the same sort of medication. I think that those who have had much experience in the treatment of lithemia agree that a nitrogenous diet is the one upon which these patients thrive best, anomalous and paradoxical as it may seem to be. As to medication, I need only refer to the renown which alkaline springs have gained all over the world for the relief of both these classes of cases. Both classes of patients go to these places, and both get relief—victims of the sour and of the sweet disease.

I quite agree with Dr. Flint as to the importance of the dietetic management of diabetic patients, and think, until we can estimate the utility of medicines independently of diet, that it is not possible for us to exactly determine their value. I think it will be found that those medicines which have attained the greatest renown in the treatment of diabetes, outside of the alkalies, have been used in connection with dietetic treatment. Another point which I think bears upon the idea I have expressed, is the fact, that if we consider the different forms of medication used in the treatment of diabetes, we will find they are those forms more or less applicable to the treatment of functional disorders of the liver in gouty subjects. Aloes has already been alluded to as a useful remedy. Strychnia has been a remedy of great renown. Alkalies certainly have been of the greatest value in the treatment of the mild, and to a certain extent in the treatment of the severer, forms of diabetes. The cases which have been described by Dr. Smith certainly lend considerable support to the treatment by opium. With this remedy I have had very little experience, generally finding that diet would ameliorate the symptoms, and never having had any success or expectation of curing these cases, have not resorted to the use of drugs for that purpose. Dr. Pavy, I know, attaches a great deal of value to the use



of codeia, but Dr. Dickinson, equally distinguished, has not had the success attending its use that has been ascribed to it by Pavy. I have had some experience with the sulphide of calcium, and have been much pleased with the result, but never use it without prescribing a strict diet, and, therefore, cannot ascribe to it its true value. In the grave forms of the disease, where the patient manufactures sugar out of animal fats, I believe no treatment does any good. Here, I believe, we have a profound nervous lesion, what it is we do not know, which will prevent the utility of any medication.

Dr. SMITH, being called upon to summarize the remarks upon his paper, said: Mr. President, I shall have very little to say in closing the discussion. Although Dr. Draper was correct in supposing that I am of the opinion that the majority of cases of diabetes have their origin in the nervous system, I am not prepared to say that the disease may not have its origin in some organ in the body. Whether it is the liver that is first involved, and then the nervous system, or the reverse, I believe we have yet to find out. Since 1877, it has seemed to me, that in this direction, there are many observations to be made, and I look for the development of interesting facts in this line. I would say, then, I am not prepared to accept the idea that the etiology is always due to disturbance of the nervous system. I was somewhat surprised at the statement of Dr. Draper, that the same diet which would relieve diabetic patients would relieve gouty patients. I believe that gouty patients cannot digest meats as well as other patients. Diabetic patients can. The same is true, I believe, with other foods. I have no doubt but that diabetic patients and gouty patients are not able to take farinaceous foods, but, beyond that, I do not think we are able to trace the analogy.

In regard to codeia, I believe no one has touched upon the special point which I tried to make, that in grave cases as well as in mild cases, codeia was indicated. In those cases in which sugar is manufactured from food not containing sugar, or made from a meat diet, I contend that codeia places the nervous system in a condition in which sugar is not produced in such quantity. As bearing upon that point, I would call attention to the history of one of the cases contained in my paper, in which, though the dietetic treatment was continued all the time, when the codeia was withdrawn, the patient became worse, and improved when its administration was again begun. I admit that this is still almost theoretical, but it illustrates practically, at least to a slight degree, a point I have made theoretically. Upon motion, the Academy adjourned.

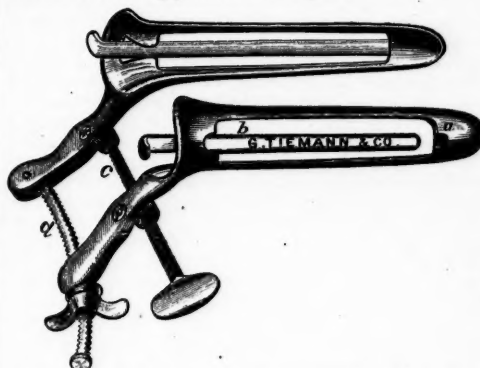
## NEW INVENTIONS.

### A NEW SPECULUM FOR THE RECTUM, AND ALSO FOR THE VAGINA IN THE VIRGIN.

BY W. W. KEEN, M.D.,  
OF PHILADELPHIA.

I HAVE found the ordinary specula so unsatisfactory in the examination of the rectum that I have devised the present one to fulfil several purposes, and I have found it practically to answer admirably the objects I have had in view. The figure shows it of about three-fifths the full size, the length of the blades being three and three-quarter inches, the breadth, seven-eighths of an inch at the base. Placing the blades and handles in apposition, the blades are dilated at the base by the screw *c*, the ends remaining in contact and so forming a long  $\Lambda$ . This enables one to expose to view the anus and lower rectum, while the closed extremity prevents the escape of the fæces, unless they be very liquid. The

value of this manœuvre in examinations for hæmorrhoids, fissures, and other anal troubles, or in any operations on piles is very obvious. If a lateral view of the rectum be desirable, one slide, *b*, or both may be withdrawn. *a* is a slot in which the extremity of the blade fits snugly when it is in place.



By screwing up the nut on the screw *d*, instead of *c*, the heel of the blades are a little dilated, but the chief dilatation is at the points. This will give a view of the rectum higher up. Or by using both screws, *c* and *d*, the blades may be dilated parallel to each other as in the engraving.

The small size of the speculum renders it admirably adapted for vaginal examinations in the virgin with the same variations in its manipulation as above described.

I must acknowledge the assistance rendered by Messrs. G. Tiemann & Co. in suggesting mechanical details which have improved this instrument.

## CORRESPONDENCE.

### HYPERMETROPIA IN THE PUBLIC SCHOOLS.

To the Editor of THE MEDICAL NEWS:

SIR: Any discussion of a medical subject which so widely affects the public interest as the condition of school life, must be, provided it be carried on with a spirit of fairness, both of interest and importance, and it is for this reason that I permit myself to add a few remarks to what has already been advanced.

I was sorry to see, from Dr. Randall's letter in THE MEDICAL NEWS, of February 4, that he was of the opinion that I meant to criticise Dr. Risley's work, published in the report on *Weak Eyes in the Public Schools*. This was not my intention, though if it had been, I had supposed that I was at liberty to do so. I certainly should have considered it a compliment if any work of mine had so impressed any of my colleagues, who had worked in the same line, as to render it worthy their serious attention and criticism.

This was not my intention, however; I merely wished to correct a statement which was not in accordance with the facts of the case, and one which Dr. Randall has himself characterized as loose. That it was a "loose statement" for a scientific paper, I do not think any one will deny.

Dr. Randall mentions the fact that I did not include in my list of foreign observers the statistics of Emmert (80.8 per cent.). This is true, for it was not necessary for the point which I wished to prove, but I am perfectly willing to do it now—and not only Emmert's enormous per cent. of 80, but also Scheiding's 25 per cent., and Koppe's 66 per cent.

This gives us eighteen foreign observers, and these

are all I know of. There may be others which I cannot find. If we add these to the former list, we then get as a combined result an average of hypermetropia equal to 21 per cent., or 1.5 times as much as my individual results; while, on the other hand, Dr. Risley's are nearly 3.5 times as large as the European average.

When, therefore, we see such tremendous discrepancies in the various examiners, for example, Dor, of Berne, 4 per cent., and Emmert, of the same place, 80 per cent., the conviction forces itself upon us that individual statistics are not of any value, and a great doubt arises in a candid mind whether the average of any and all are of any worth, so far as hypermetropia is concerned. When, moreover, we see such a careful and competent observer as Becker getting 12 per cent., and Emmert 80 per cent., the suspicion arises at once that the fault is not with the eyes examined, as much as with the method of examination. No one believes for a moment that when Dr. Derby and I get 13 per cent., and Dr. Risley 75 per cent., that there is six times as much hypermetropia in Philadelphia as in New York. Nor does this immense difference lie in errors made either on my part or on Dr. Risley's. I am quite sure if I had made my examinations in Philadelphia, I should have got a very much lower average, while Dr. Risley would have got a very much higher one here; and I feel quite convinced that some of the observers here would get 70 or 80 per cent. every time where I got 15 or 20 per cent.

These immense variations must strike an outsider, who is accustomed to look upon this branch of medicine as the most exact there is, as something preposterous; but, fortunately, it is not as bad as it looks, and depends mostly on the methods used, and the conception of what refraction really is, or rather what an emmetropic eye is. Strictly speaking, emmetropia is a mathematical point, and the slightest variation to either side of this makes either hypermetropia or myopia, and it is these small and trifling deviations which make the difference appear so large.

This brings me to a consideration of a point which I allude to all the more willingly as it was introduced by Dr. Randall, who observes in his letter that, while I give two of Cohn's examinations, where the percentage was 3 and 7, I omit his examination under atropia, where the percentage was over 88 per cent., "an omission no doubt accidental." On the contrary, the omission was intentional, as I should have been sorry to have made the mistake of comparing statistics made with atropia and those made without. The reason is that atropia always produces a temporary and artificial hypermetropia in the normal emmetropic eye, which, according to Donders, varies from  $\frac{1}{15}$  to  $\frac{1}{10}$ . I alluded to this as long ago as 1866, and again in Philadelphia, in 1876, and Dr. Wadsworth, and Dr. Knapp, and others have called attention to it. Now, when we turn to Cohn's statistics in the original, we find that not only "was the percentage over 88," but that it was over 99 per cent., and that there was not a single emmetropic eye left. Cohn himself says, and he prints it in the largest possible type, "*Jeder scheinbare Emmetrop war nach Atropin hyperopisch*."—"Every eye apparently emmetropic was after atropia hyperopic." The only fair way, then, when atropia is used, is to discount the effect of the drug, and the question is what amount to allow for. I myself have taken  $\frac{1}{10}$ , which is about the medium between the extreme estimates made by Donders. It is supposed that this artificial hypermetropia is due to flattening of the lens; and this would appear to be corroborated by the examination made by Woinow, who found the radius of curvature of the lens of an eye, examined under atropia by the ophthalmometer, to be greater after its use than before. 9.74 mm. before, 10.10 after.

Then, again, some definite standard should be used. Dr. Derby and myself used, as stated in our work,  $\frac{1}{10}$ , either plus or minus, as the lowest variation from emmetropia which we thought would give an approximately fair result with school-children, and even then it often occurred that children with vision as sharp or sharper than the normal would still read Snellen xx. at twenty feet through  $\frac{1}{10}$ , who were not hyperopic but emmetropic with the ophthalmoscope, and who, when tried, would always choose a plane glass in preference to the weak convex one. The mere fact that a young child sees xx. Snellen through a weak convex glass, does not show that it is hyperopic, as it may do this under weak circles of dispersion. Emmert himself says that out of his 80 per cent. "92 per cent. were hyperopic between  $\frac{1}{12}$  and  $\frac{1}{10}$ , and that a great part, perhaps the greater part, are to be looked upon not as *pathologically*, but as *physiologically* hyperopic, and if we suppose a certain amount of negative accommodation, then a great quantity of the cases of *apparent* hypermetropia will be explained," and I cannot help thinking that this is the explanation of the phenomenal percentage that he and others get; and he further explains "that we have, therefore, an *accommodative* hyperopia, which should be distinguished from *actual* hyperopia—that is, a *departure from the normal form; a want of development*." It was, I must admit, the actual, that is, a shortening of the axis, that I was after, and which I tried to obtain in my statistics, and I prefer to consider an eye which has no anatomical defect an emmetropic eye, rather than a "physiologically hypermetropic one," whatever that may mean. But some of my friends do not.

But to return to the standard. Emmert used  $\frac{1}{12}$  as his limit. Cohn in some cases  $\frac{1}{10}$ , and in those under atropia,  $\frac{1}{15}$ . No wonder all his emmetropic cases showed H under atropia, especially as some of his children had a vision three times the normal, according to Snellen; a large quantity twice the normal; all over the normal except seven, and these seven the full amount. Dr. Derby and myself used  $\frac{1}{10}$ . It is not stated in Dr. Risley's elaborate report what standard he used, and no estimate can be formed of what the limit was, either by glasses or the ophthalmoscope.

Thus I would propose that when examinations are to be made in future, that some definite standard should be agreed upon, and that the effect of atropia, when used, should be accounted for, or, if not, then that every degree of error should be noted.

In my former letter I asserted that the same methods and same precautions were taken in Dr. Derby's and my examination and those of Dr. Risley. I find now that there was, however, a marked difference, for although the trial-glasses and ophthalmoscope were used in both cases, still they were differently employed. In my case, *every* eye was first examined with glasses, and its refractive condition noted, and then, without the result being known, it was examined with the ophthalmoscope. The two results were then compared, and if a discrepancy existed, a fresh examination was made, and in cases of any doubt, the preference was given to the glasses. This only occurred in very slight degrees. Thus, my results are practically a test by glasses. Dr. Risley's cases, on the contrary, were examined by the ophthalmoscope, and the refraction determined by that, with the exception that trial-glasses were used in doubtful cases. His results, therefore, are practically a test by the ophthalmoscope.

Now, as to the ophthalmoscope, with every incentive to look favorably upon it, and, while I consider it an indispensable agent in determining the nature of refraction, candor compels me to declare that I cannot consider the instrument as accurate a test as that by glasses. The "variables" are too many and too great

to make it, in my opinion, a safe guide when used alone in the lesser errors, a fact which I have always recognized and commented upon in various places, as, indeed, have others.

There are, of course, many things in Dr. Randall's letter which, were it of the slightest use, I might reply to, but discussions in regard to statistics, which seem to vary like the wind, are as endless as they are fruitless. I should, however, before taking leave of the subject, say that while I disagree in regard to the frequency of the occurrence of the various errors in refraction, notably the large amount of hypermetropia, I also disagree as to the phenomenal amount of conus in the normal as well as the abnormal eye. Thus, we find from table VI., in Dr. Risley's report, the percentage of conus, beginning at the enormous amount of 43.4 per cent. in emmetropic eyes, at eight years, and falling to 6.5 per cent. at seventeen; in the hyperopic eyes, from 41 to 22; and in the myopic, from 64 to 53. As conus is an anatomical change, the question arises, Where did the 37 per cent. of decrease in the emmetropic eye, for example, go to? It is stated that this frequency of conus, among other points, "was a genuine surprise to the Committee," and it certainly is to me, and I think will be to the ophthalmological world in general. Now, although there are these, and some other, points about which I disagree, there are many others, indeed most of the important ones, with which I not only agree, but to the truth of which, I, with others, have already testified at various times, in as an emphatic way as was possible.

I am, respectfully yours,

EDWARD G. LORING.

NEW YORK, February 9, 1882.

## NEWS ITEMS.

### WASHINGTON.

(From our Special Correspondent.)

A MUSEUM OF HYGIENE.—The action of the American Public Health Association, which makes the Bureau of Medicine and Surgery of the United States Navy a permanent central repository for such material as may be placed in a museum of hygiene, promises to open up a new and important educational branch. Space has already been set apart for the purpose, directly under the control of Medical Inspector A. L. Gihon; and Surgeon-General Wales has recently issued a comprehensive circular asking for contributions and for the co-operation of all interested in sanitary matters, to make this attempt a success.

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### BUFFALO.

(From our Special Correspondent.)

A STATEMENT has been going the rounds of the newspapers concerning the unhealthfulness of Buffalo, which is entirely without foundation in fact. The city is now, as it has always been, one of the healthiest towns of its size in the country. The mortality of Buffalo proper, is small, but some of its suburbs are in a very bad hygienic condition from the presence of slaughter houses, oleomargarine factories, and from cattle and pig-pens. In these localities, from four to five miles from the centre of the city, there is a good deal of disease from local causes, which the Board of Health is now trying to obviate.



are all I know of. There may be others which I cannot find. If we add these to the former list, we then get as a combined result an average of hypermetropia equal to 21 per cent., or 1.5 times as much as my individual results; while, on the other hand, Dr. Risley's are nearly 3.5 times as large as the European average.

When, therefore, we see such tremendous discrepancies in the various examiners, for example, Dor, of Berne, 4 per cent., and Emmert, of the same place, 80 per cent., the conviction forces itself upon us that individual statistics are not of any value, and a great doubt arises in a candid mind whether the average of any and all are of any worth, so far as hypermetropia is concerned. When, moreover, we see such a careful and competent observer as Becker getting 12 per cent., and Emmert 80 per cent., the suspicion arises at once that the fault is not with the eyes examined, as much as with the method of examination. No one believes for a moment that when Dr. Derby and I get 13 per cent., and Dr. Risley 75 per cent., that there is six times as much hypermetropia in Philadelphia as in New York. Nor does this immense difference lie in errors made either on my part or on Dr. Risley's. I am quite sure if I had made my examinations in Philadelphia, I should have got a very much lower average, while Dr. Risley would have got a very much higher one here; and I feel quite convinced that some of the observers here would get 70 or 80 per cent. every time where I got 15 or 20 per cent.

These immense variations must strike an outsider, who is accustomed to look upon this branch of medicine as the most exact there is, as something preposterous; but, fortunately, it is not as bad as it looks, and depends mostly on the methods used, and the conception of what refraction really is, or rather what an emmetropic eye is. Strictly speaking, emmetropia is a mathematical point, and the slightest variation to either side of this makes either hypermetropia or myopia, and it is these small and trifling deviations which make the difference appear so large.

This brings me to a consideration of a point which I allude to all the more willingly as it was introduced by Dr. Randall, who observes in his letter that, while I give two of Cohn's examinations, where the percentage was 3 and 7, I omit his examination under atropia, where the percentage was over 88 per cent., "an omission no doubt accidental." On the contrary, the omission was intentional, as I should have been sorry to have made the mistake of comparing statistics made with atropia and those made without. The reason is that atropia always produces a temporary and artificial hypermetropia in the normal emmetropic eye, which, according to Donders, varies from  $\frac{1}{10}$  to  $\frac{1}{6}$ . I alluded to this as long ago as 1866, and again in Philadelphia, in 1876, and Dr. Wadsworth, and Dr. Knapp, and others have called attention to it. Now, when we turn to Cohn's statistics in the original, we find that not only "was the percentage over 88," but that it was over 99 per cent., and that there was not a single emmetropic eye left. Cohn himself says, and he prints it in the largest possible type, "*Jeder scheinbare Emmetrop war nach Atropin hyperopisch*."—"Every eye apparently emmetropic was after atropia hyperopic." The only fair way, then, when atropia is used, is to discount the effect of the drug, and the question is what amount to allow for. I myself have taken  $\frac{1}{6}$ , which is about the medium between the extreme estimates made by Donders. It is supposed that this artificial hypermetropia is due to flattening of the lens; and this would appear to be corroborated by the examination made by Woinow, who found the radius of curvature of the lens of an eye, examined under atropia by the ophthalmometer, to be greater after its use than before. 9.74 mm. before, 10.10 after.

Then, again, some definite standard should be used. Dr. Derby and myself used, as stated in our work,  $\frac{1}{6}$ , either plus or minus, as the lowest variation from emmetropia which we thought would give an approximately fair result with school-children, and even then it often occurred that children with vision as sharp or sharper than the normal would still read Snellen xx. at twenty feet through +  $\frac{1}{6}$ , who were not hyperopic but emmetropic with the ophthalmoscope, and who, when tried, would always choose a plane glass in preference to the weak convex one. The mere fact that a young child sees xx. Snellen through a weak convex glass, does not show that it is hyperopic, as it may do this under weak circles of dispersion. Emmert himself says that out of his 80 per cent. "92 per cent. were hyperopic between  $\frac{1}{2}$  and  $\frac{1}{6}$ , and that a great part, perhaps the greater part, are to be looked upon not as *pathologically*, but as *physiologically* hyperopic, and if we suppose a certain amount of negative accommodation, then a great quantity of the cases of *apparent* hypermetropia will be explained," and I cannot help thinking that this is the explanation of the phenomenal percentage that he and others get; and he further explains "that we have, therefore, an *accommodative* hyperopia, which should be distinguished from *actual* hyperopia—that is, a *departure from the normal form*; a *want of development*." It was, I must admit, the actual, that is, a shortening of the axis, that I was after, and which I tried to obtain in my statistics, and I prefer to consider an eye which has no anatomical defect an emmetropic eye, rather than a "physiologically hypermetropic one," whatever that may mean. But some of my friends do not.

But to return to the standard. Emmert used  $\frac{1}{3}$  as his limit. Cohn in some cases  $\frac{1}{6}$ , and in those under atropia,  $\frac{1}{6}$ . No wonder all his emmetropic cases showed H under atropia, especially as some of his children had a vision three times the normal, according to Snellen; a large quantity twice the normal; all over the normal except seven, and these seven the full amount. Dr. Derby and myself used  $\frac{1}{6}$ . It is not stated in Dr. Risley's elaborate report what standard he used, and no estimate can be formed of what the limit was, either by glasses or the ophthalmoscope.

Thus I would propose that when examinations are to be made in future, that some definite standard should be agreed upon, and that the effect of atropia, when used, should be accounted for, or, if not, then that every degree of error should be noted.

In my former letter I asserted that the same methods and same precautions were taken in Dr. Derby's and my examination and those of Dr. Risley. I find now that there was, however, a marked difference, for although the trial-glasses and ophthalmoscope were used in both cases, still they were differently employed. In my case, *every* eye was first examined with glasses, and its refractive condition noted, and then, without the result being known, it was examined with the ophthalmoscope. The two results were then compared, and if a discrepancy existed, a fresh examination was made, and in cases of any doubt, the preference was given to the glasses. This only occurred in very slight degrees. Thus, my results are practically a test by glasses. Dr. Risley's cases, on the contrary, were examined by the ophthalmoscope, and the refraction determined by that, with the exception that trial-glasses were used in doubtful cases. His results, therefore, are practically a test by the ophthalmoscope.

Now, as to the ophthalmoscope, with every incentive to look favorably upon it, and, while I consider it an indispensable agent in determining the nature of refraction, candor compels me to declare that I cannot consider the instrument as accurate a test as that by glasses. The "variables" are too many and too great

to make it, in my opinion, a safe guide when used alone in the lesser errors, a fact which I have always recognized and commented upon in various places, as, indeed, have others.

There are, of course, many things in Dr. Randall's letter which, were it of the slightest use, I might reply to, but discussions in regard to statistics, which seem to vary like the wind, are as endless as they are fruitless. I should, however, before taking leave of the subject, say that while I disagree in regard to the frequency of the occurrence of the various errors in refraction, notably the large amount of hypermetropia, I also disagree as to the phenomenal amount of conus in the normal as well as the abnormal eye. Thus, we find from table VI., in Dr. Risley's report, the percentage of conus, beginning at the enormous amount of 43.4 per cent. in emmetropic eyes, at eight years, and falling to 6.5 per cent. at seventeen; in the hyperopic eyes, from 41 to 22; and in the myopic, from 64 to 53. As conus is an anatomical change, the question arises, Where did the 37 per cent. of decrease in the emmetropic eye, for example, go to? It is stated that this frequency of conus, among other points, "was a genuine surprise to the Committee," and it certainly is to me, and I think will be to the ophthalmological world in general. Now, although there are these, and some other, points about which I disagree, there are many others, indeed most of the important ones, with which I not only agree, but to the truth of which, I, with others, have already testified at various times, in as an emphatic a way as was possible.

I am, respectfully yours,

EDWARD G. LORING.

NEW YORK, February 9, 1882.

## NEWS ITEMS.

### WASHINGTON.

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## INDIANAPOLIS.

(From our Special Correspondent.)

SMALL-POX has made its appearance in this city, two cases having been reported up to date. Stringent measures are being enforced by both the State and City Boards of Health, and the City Councils have passed an ordinance requiring every person who has not been vaccinated within the last ninety days to be vaccinated within the next ten days or be subject to fine. They also impose a fine upon any one having small-pox who does not hang out a red flag. An amusing instance of this occurred a few days ago. It was found that a case of small-pox was secreted on one of the principal streets and no flag was hung out. An order for the immediate arrest of the parties was issued, but the police were loth to serve it. The Mayor also thought it would be better policy to wait until the party recovered, rather than bring him out and subject the people to the contagion. A red flag is displayed at his residence now, but whether the family or the police put it up will ever be a mystery.

TYPHOID FEVER was not prevalent here as it was in other parts of the State, only a few cases having been reported. Its ravages were fearful in some of the rural districts. At Milroy, in Rush County, a town of three hundred inhabitants, about seventy-five cases appeared in the town in an area of two miles in diameter. Only six or seven cases died, however. Similar ravages are reported from all quarters of the State.

THE STATE BOARD OF HEALTH was fully organized January 1, with Dr. Thad. M. Stevens, Secretary and State Health Officer. The County Boards were also put in working order the first of the year. The Board issued an order that every person within the State must be vaccinated before the first day of February or be fined five dollars.

THE MEDICAL COLLEGE OF INDIANA has the largest class ever in attendance. The college building is new, and one of the best equipped in the West.

## VIENNA.

(From our Special Correspondent.)

SURGICAL NOTES.—Prof. Theodor Billroth has recently returned from St. Petersburg, where he was called to amputate the leg of Prince Czertkow. His fee was 15,000 florins, or about \$6300.

Billroth has adopted the antiseptic method of Lister in a very modified form. He uses no spray apparatus, but in its stead floods the site of an operation with a 3 per cent. solution of carbolic acid in water. Instruments and the hands of the assistants are washed in a 5 per cent. solution. Catgut ligatures are employed. The universal antiseptic dressing for every form of wound is iodoform, in powder or gauze. This dressing was introduced into Vienna about one year ago by Mosetig and Johann Mikulicz, formerly Billroth's first clinical assistant and now privat-docent für chirurgie. The gauze is a coarse-meshed cotton fabric, deprived of vegetable oils; its meshes are filled with powdered iodoform, very much in the manner crinoline is treated with plaster of Paris to make a "plaster" roller.

"Immediately after the introduction of iodoform as an antiseptic remedy," says Billroth, "Viennese physicians and surgeons went to extremes and employed the remedy in large doses, internally and externally, for every form of disease and in every condition of life. Disastrous results, such as meningitis in children, albuminuria in adults, melancholia, febrile disturbances of serious character, collapse, at once appeared as the effects of this absurd practice. The remedy, then, was unreasonably totally rejected. Now, a healthy reaction

in medical thought is setting in, and iodoform is assuming its proper position as a local antiseptic remedy, an invaluable adjuvant to carbolic acid—not, as König, of Göttingen, or Scheder, of Hamburg, says, a substitute." The odor of iodoform is well concealed by the bean, "tonka bona."

THE PUBLIC HEALTH.—The reports for the week ending February 18, so far as received, show the following returns of deaths from small-pox: Philadelphia, 17; Allegheny, 16; Huntingdon, Pa., 1; Montclair, N. J., 1; Cincinnati, 31; Milwaukee, 1. The disease has increased in Cincinnati, remained nearly stationary in Philadelphia, and decreased slightly in Hudson County, New York. Diphtheria is at present quite prevalent in Boston, 14 deaths from this cause being reported in the week ending February 18. In the same week there were 15 deaths from this cause in Philadelphia; 1 in Providence; 3 in Hudson County, New York; 2 in Cincinnati; and 1 in Milwaukee. Scarlet fever prevails in Hudson County, New York, there being 8 deaths reported there, against the same number in Philadelphia, 2 in Boston, 3 in Cincinnati, and 1 in Milwaukee. Typhoid fever caused 21 deaths in Philadelphia; 4 each in Cincinnati and Wheeling, W. V.; and 2 each in Hudson County, Boston, Richmond, and Milwaukee. One case of cerebro-spinal meningitis is reported from Milwaukee; and whooping-cough and measles are almost entirely absent from the mortuary records. No unusual prevalence of acute lung diseases is to be noted.

HEALTH IN MICHIGAN.—The reports to the State Board of Health for the week ending February 11, 1882, indicate that scarlet fever and tonsillitis have considerably increased, and that rheumatism, bronchitis, intermittent fever, diarrhoea, and neuralgia, have considerably decreased in area of prevalence. Special reports have been received, showing small-pox present during the week ending February 11, and since, at eleven places in Michigan, as follows: At Kalamazoo, Detroit, and Marshall, February 4; in Port Huron Township, February 5; in White Cloud, Newaygo County, February 6; in Kimball Township, St. Clair County, February 7; at Saginaw City (one case), February 7; at St. Joseph, on vessel (one case), February 11; at Spalding, Menominee County (eight cases, white), February 13; among the Indians at Indian Town, near Spalding (sixteen deaths and six cases up to), February 13; at Grand Rapids, February 14; at Parkville, St. Joseph County (one, convalescent), February 15, 1882.

HEALTH OF MILWAUKEE.—We are glad to learn that Milwaukee, although only 85 miles by rail from Chicago, with her death-rate of 134 for January, has had but 5 cases of small-pox.

THE NEW LIBRARY AND MUSEUM BUILDING FOR THE SURGEON-GENERAL'S OFFICE.—Dr. J. S. Billings, United States Army, in charge of the library of the Surgeon-General's office, appeared last Saturday before a sub-committee of the House Committee on Public Buildings and Grounds to-day and urged the necessity for a new building for the library, museum, and records of the Surgeon-General's office.

PROGRESS OF MEDICINE IN CHINA.—The late lamented Dr. Osgood completed, shortly before his death, a translation of Gray's *Anatomy* into the Chinese. He was threatened with brain disease for several months, and died suddenly while the work was passing through the press. As the Chinese have no ideas of anatomy, except what are purely conjectural, it was no simple task to make a translation, as special terms had to be introduced, and these again explained in a vocabulary.



Such a work will be of inestimable value to the native student, and have a tendency to open the eyes of others, in regard to the foolish conjectural teachings of their ancient sages.

Foreign physicians have now produced, for the Chinese, works upon anatomy, *materia medica*, chemistry, obstetrics, and several minor subjects. Dr. John G. Kerr, since his return to Canton, has entered upon the work left unfinished by Dr. Osgood, and is engaged, with the aid of two natives, who are all the time occupied, in making translations of other medical treatises.

Several physicians have recently arrived in China, to be located in Nanking, the Province of Canton, Swatow, and Formosa.

**QUACKERY WITHIN THE PROFESSION.**—The profession is probably unaware of the progress steadily made by medical quackery in its diverse forms and disguises. Quackery which is not medical—in the sense of being practised by duly qualified men—is undoubtedly an evil, but its consequences are not comparable with the effects of such quackery as is growing apace within our own ranks, and slowly it may be, but surely, undermining the respect and confidence which the profession has hitherto deserved and received from the public. We sometimes wonder that our calling does not command the warm recognition in certain quarters to which it seems entitled. For a sufficient explanation of this default in the estimation of society, let us look to the prevailing and almost daily increasing popularity of "systems" and "cures" tacitly, if not avowedly, supported or countenanced by the profession. There is a sentimental and mock-heroic spirit abroad which burlesques the candor of "truth-seeking," and even mimics the impulses of chivalry. We hesitate to condemn any system, "lest there should be some good in it," and we are too tender-hearted and polite to deal honestly by its promoters, even though we recognize the fallacy of their pretensions, and more than suspect their motives. This is not a faithful line of conduct in reference to our profession, nor is it loyal to science, which is one of the many constituent parts and aspects of truth. We know, or ought to know, that a perfectly just and truthful conception of the science of medicine must bar the recognition of *systems* and *cures* of any class or description. The art of healing is not a system, and can never be made one. It is simply an intelligent application of the laws of health in the remedy of disease. We study the "symptoms" of a malady with a view to the acquisition of precise knowledge as to its nature, course, and rational treatment. We pursue the investigation of disease over the boundary-line of death, and explore the cadaver with a view to ascertain the effect of the morbid state on the organism and to elicit its organic causes, albeit we too commonly confound effects with causes. We test the powers and analyze the constitution of drugs, and we scrutinize and make careful trial of methods of treatment, to obtain a reasonable acquaintance with their natures and actions. In brief, we take any amount of trouble and resort to every means at our disposal to render the principles and practice of our art *rational*. This is our duty, and it is the only method consistent with self-respect and professional integrity; but, if side by side with this policy, we cherish a spirit of credulity which renders us ever ready to countenance systems of which we can know nothing—because there is nothing to know—and take a false pride in showing friendliness to quacks and charlatans, the good work we ourselves may do is changed to evil by reason of the actual or implied sanction we give to the bad work done by others. Nothing is so much needed just now as the rise in our midst of a stern and uncompromising apostle of sincerity in science—a man of unpying animosity to humbug in

all its forms, who will not hesitate, at any bidding, to denounce wrong-doing and untruthfulness, let who may be the offender. It is time that a spirit of manliness went out in our ranks to chase away the lying spirit of mock courtesy—the faint-hearted and time-serving sentimentality—which makes us so ready to look kindly on any pretender, and so reluctant to expose any pretense.

There cannot possibly be a "system" or "cure" in medicine. There are no rule-of-thumb methods and no *mysteries* in true science. If we do not know what a remedy is, and how it acts, we have no right, as honest men, to employ it. The time has passed for the working of cures by charms and the recourse to nostrums. We pander to the credulity of the unskilled community when we show ourselves credulous. We patronize and encourage quackery when we extend professional recognition to a quack. Every man is a quack—whether qualified or unqualified—who employs a remedy without knowing why, or who adopts a "system" in medicine. The profession must speak out clearly and strongly on this point, and without delay. From the highest places in society to the lowest ranks of the people, there is just now a grievous readiness to "believe in" quacks and quackery. We have ourselves to thank for this most adverse "feeling" and "influence." It is the stirring of the viper we have brought in from the cold, where physicians and surgeons of more robust intelligence than those of to-day left it—the viper we have warmed and fed and brought back to life; and now it is preparing to rise and sting the hand that caressed it. The way to encounter the charlatanism which is making head against science is to be at once more candid and more conspicuously *honest* in our dealings with the public. We must lay aside the last vestige of the robe of mystery, and show by our words and works, our conduct and policy, that medicine is not a science that admits of inspiration, and that the practice of healing is not an art which can be acquired by the unlearned. There is no system or cure, or charm or nostrum, known to the profession; our calling consists solely in the rational study and treatment of disease on common-sense principles. For those who pretend to a sort of inspiration we have no professional friendship; and towards the promoters of systems and 'pathies we can have no leaning, or any feeling other than that of suspicion, if not pity and contempt. They can have no place in our professional intercourse, and we can have nothing to say to them or their work. This is the only sentiment worthy of the medical profession in its dealings with medical quacks, and the time has come when the revival of its old spirit is most earnestly to be desired.—*Lancet*.

**HOMŒOPATHY AND THE BRITISH MEDICAL ASSOCIATION.**—The Southwestern Branch of the British Medical Association does not, apparently, intend to allow the question of the relations existing between homœopathic practitioners and the Association to remain in its present unsatisfactory state. The subjoined resolutions were passed unanimously at the last quarterly meeting of the Branch, held at Plymouth, on December 31, under the presidency of Dr. Hudson, of Redruth:

"1. That this meeting desires to express its entire disapproval of the views, in relation to consultations with homœopathic practitioners, expressed by the readers of Addresses in Medicine and Surgery at the annual meeting of the Association at Ryde, in 1881.

"2. That this meeting desires to direct the attention of the Committee of Council of the Association to the resolutions in regard to homœopathic practitioners passed at the annual meeting of the Association in 1852, and reaffirmed at the annual meetings of 1858 and 1861;

and now calls upon the Committee of Council to put in force as speedily as possible By-law 3 against homeo-paths and all members of the profession who assume designations implying the adoption of special modes of treatment."—*Lancet*, Feb. 4, 1882.

**THE BURIAL-PLACE OF HARVEY.**—The tower of the old church at Hempstead, in Essex, England, in which William Harvey is buried, fell down on the evening of Saturday, January 28, bringing with it nearly one-half of the roof and one arch from the south side of the nave, and letting down a good part of the roof of the south aisle. The vault in which Harvey lies is at the northwestern angle of the church, and is, consequently, uninjured. The church is supposed to have been built between the ninth and eleventh centuries. The medical profession of England and America are invited to subscribe towards the restoration of the building, which it is estimated will require about £5000. Subscriptions may be sent to Dr. B. W. Richardson, London.

**VACCINE APPLIED TO SHEEP.**—A Paris despatch to the London *Times* states that at a farm near Melun experiments were recently made by M. Pasteur in the presence of a host of specialists on the duration of the action of anthracic vaccine as applied to sheep. It will be remembered that six months ago M. Pasteur vaccinated a number of sheep with anthracic vaccine, the immediate result being to preserve all these sheep from anthracic virus, whereas sheep not so vaccinated succumbed within twenty-four hours to the latter. The question was how long the influence of such vaccine would last. The new experiments proved that it lasts six months, and they will be continued from month to month to ascertain the exact duration of the preservative. Four unvaccinated sheep were inoculated with anthracic virus, as also four of the sheep vaccinated six months ago. Two of the unvaccinated sheep expired within twenty-four hours, and the other two subsequently, whereas the sheep vaccinated six months ago admirably resisted the action of the virus. Another curious fact was ascertained. A lamb, the offspring of a vaccinated sheep, was inoculated with the virus. It expired within twenty-four hours, thus proving that the vaccine virtue is not transmitted hereditarily.

The Seine-et-Marne Agricultural Society presented M. Pasteur with a gold medal, and a banquet was held at which the great service rendered to agriculture by his discovery was warmly testified to.

**MEDICAL DIPLOMAS IN FRANCE.**—The six Medical Faculties in France have conferred the following diplomas during the school year 1880-81: The Faculty of Paris, 461; of Montpellier, 66; of Lyons, 44; of Nancy, 19; of Bordeaux, 18; and of Lille, 13. Total, 621.—*Med. Times and Gaz.*, Jan. 14, 1882.

**HOSPITAL SUNDAY IN LONDON.**—The collections this year amounted to \$150,735, of which the Church of England gave eleven-fifteenths. All denominations—Jews, Greeks, Friends, etc.—are represented in the collection.

**LECTURES OF THE ROYAL COLLEGE OF PHYSICIANS.**—The following are the lectures of the Royal College of Physicians for the present year: Gulstonian Lectures, by William Ewart, M.B., March 3, 8, and 10, on "Pulmonary Cavities, their Origin, Growth, and Repair;" Croonian Lectures, by Sir Joseph Fayrer, K.C.S.I., M.D., March 15, 17, and 22, "On the Climate and Fevers of India;" Lushington Lectures, by J. Burdon Sanderson, M.D., March 24, 29, and 31, "On the Pathology of Inflammation."—*British Med. Journ.*, Jan. 21, 1882.

**OBITUARY.**—Died, in Cologne, January 7, in the seventy-second year of his age, THEODOR SCHWANN. Schwann was born in Neuss-on-the-Rhine in 1810; was Professor of Physiology in Löwen from 1838 to 1848; and from 1848 up to the time of his death, Professor in Liège. With the name of Schwann, above all others, is associated the first conception of the cell-doctrine, the foundation and starting-point of modern physiology and pathology; and, although he distinguished himself in other work, it is as the founder of the cell-doctrine, as shown in his work on the *Structure and Mode of Growth of Plants*, published in 1839, that he will be remembered. From 1834 to 1839 he was the assistant of Johannes Müller.

—At Brooklyn, on February 12, THEODORE L. MASON, M.D., aged seventy-nine years. Dr. Mason graduated at the College of Physicians and Surgeons, New York, in 1825, and was for many years prominent in the affairs of the Long Island College Hospital, and of the Inebriate Asylum of Kings County.

—In this city, on the 20th instant, aged seventy-six, ROBERT BRIDGES, M.D., Emeritus Professor of Chemistry in the Philadelphia College of Pharmacy, and formerly President of the Academy of Natural Sciences, of Philadelphia.

## NOTES AND QUERIES.

### IS TYPHOID FEVER CONTAGIOUS?

To the Editor of THE MEDICAL NEWS:

SIR: Two months ago typhoid fever was imported into this village, at a time when there was not another case probably within five miles. Two weeks thereafter cases began to occur, first within it and then around it, now numbering about eighteen, and others are still arising. We have no other epidemic disease, nor have we had any recently. No other probable cause having been discovered, we desire to know whether it has been propagated by contagion.

Do the urine, and the perspiration, and the saliva, etc., as well as the excreta from the bowel of the patient according to Sir William Jenner, contain its specific poison? If they do, in what stage does it first appear, and when does it disappear? and are its poisonous properties destroyed by boiling water? Any information that you may give, and especially respecting its contagious nature, will perhaps be gratefully received by many of your readers. To invite the professional mind to repeated reflections on the character of a disease so universal and fatal, is to do no more than its importance requires.

Very respectfully,

T. G. WITTEN.

TAZEWELL COURT-HOUSE, MD., Feb. 7, 1882.

[Our correspondent's case is instructive. The typhoid germs—probably contained in the excreta—admitted to the soil and ground water, became active, and hence the subsequent cases. Typhoid is a miasmatic infection, rather than a contagious disease. Some of our correspondent's inquiries cannot be answered in the present state of our knowledge. That boiling water destroys the vitality of typhoid-fever germs, seems highly probable.—ED.]

### OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT, U. S. ARMY, FROM FEBRUARY 14, TO FEBRUARY 20, 1882.

MAUS, L. M., *Captain and Assistant Surgeon*.—The leave of absence granted him in S. O. 222, A. G. O., October 1, 1881, extended one month.—S. O. 36, A. G. O., February 14, 1882.

BANISTER, JOHN M., *First Lieutenant and Assistant Surgeon*.—Fort Reno, Indian Territory. The leave of absence granted him in January 1, S. O. 18, Department of the Missouri, January 24, 1882, is extended one month.—S. O. 16, *Military Division of the Missouri*, February 15, 1882.

THE MEDICAL NEWS will be pleased to receive early intelligence of local events of general medical interest, or which it is desirable to bring to the notice of the profession.

Local papers containing reports or news items should be marked. Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

All communications relating to the editorial department of the NEWS should be addressed to No. 1004 Walnut Street, Philadelphia.